THURSDAY, SEPTEMBER 21, 1876

ZITTEL'S HANDBOOK OF PALÆONTOLOGY

Handbuch der Palæontologie. Unter Mitwirkung von W. Ph. Schimper, Professor an der Universität in Strasburg. Herausgegeben von Karl A. Zittel, Professor an der Universität in München. Erster Band. Erste Lieferung; mit 56 Original-holzschnitten. Munich, 1876: R. Oldenbourg. (London: Williams and Norgate.)

A WORK bearing on its title-page the well-known names of the Professor of Geology in the University of Strasburg, and the Director of the Royal Palæontological Museum at Munich would, under any circumstances, command attention, but one addressed to so large and so varied a circle as that for which a handbook or text-book is ordinarily designed, becomes, under such auspices, especially noteworthy, and although the first part only has as yet appeared, it can scarcely be thought too soon to bring it under the notice of such of the readers of NATURE as do not habitually see Continental scientific publications. In few departments of science does there exist the same need for a modern text-book as in general palæontology, especially for a treatise suited to the requirements of the student—something broader in scope than the fashionable "science primers" on the one hand, and without the elaborate details of special memoirs on the other—but it is at the same time true that the satisfactory preparation of a manual embracing so wide a subject needs qualifications of an unusual sort. The opening part of the present work has therefore a hearty reception assured to it, nor if succeeding instalments bear out the promise of the first, can any reasonable anticipations be disappointed. The authorship of the book is to be apportioned in the following manner. It is to be completed in two volumes, the first of which, devoted to Palæozoology and containing also the Introductory matter is entirely in the hands of Prof. Zittel. Of the second volume, one division, that on Palæophytology, is undertaken by Prof. Schimper, and the other, on Historical Palæontology by Prof. Zittel.

The part before us commences with a chapter of Preliminary Notions, and then one on Geological Succession; these are followed by a very interesting Historical Summary of palæontological discovery, from the earliest allusions to fossils in the writings of the Greek historians, down to the present day. A fourth section is devoted to Biological Considerations, and this is succeeded by a classified Bibliographic List of standard works in the several departments of the subject. These together form the Introduction, and little need be said concerning it, except that it is all well done. It is worth while to quote a single passage from the biological section which well deserves the leaded type in which it is printed—it is on the question of "Species," and runs thus (page 46—the italics are our own) "All those individuals, or remains of individuals, are regarded as belonging to one species, which have a number of constant characters in common, and which, *independent of distribution in space or time*, constitute, as a whole, a well-defined form-group, which indeed may be connected by many passage forms (but not completely) with other form-groups." An excellent and practical definition in the face of the prevalent custom of re-naming the same zoological form every time it appears in a new area, or at a fresh geological horizon, and one worth enforcing, if it were only in the interests of the next generation of palæontologists. Were the principle embodied in it generally adopted, the exercise of common sense in the estimation of the biological significance of minor characters would be sufficient to clear our fossil-lists of hundreds, nay of thousands, of the superfluous specific names with which they are at present crowded.

Systematic Palæontology is introduced by an outline of zoological classification based on the arrangement employed by Claus in his "Grundzüge der Zoologie," in which the animal kingdom is primarily divided into seven sub-kingdoms—PROTOZOA, CŒLENTERATA, ECHINODERMATA, VERMES, MOLLUSCA, ARTHROPODA, and VERTEBRATA. The portion of the Handbook now issued treats of the first of these—the Protozoa. This group is subdivided into three classes, MONERA, RHIZOPODA, and INFUSORIA. The Monera are but slightly represented amongst known fossils, and the Infusoria not at all, so that, practically, the part amounts to a synopsis of fossil Rhizopoda. Sponges, it is to be noted, are referred to the Cœlenterata, of which sub-kingdom they form the lowest section.

The MONERA are sufficiently treated in a few pages embodying a brief summary of the various researches on Coccoliths, Coccospheres, Cyatholiths, Discoliths, and other microscopical bodies of which the precise significance may still be regarded as more or less *sub judice*.

The class RHIZOPODA is subdivided into three orders—*Foraminifera*, *Radiolaria*, and *Lobosa*, the last-named having, of course, no fossil representatives. The *Foraminifera* are described, in brief, as Rhizopoda with many-chambered or single-chambered calcareous, or less frequently arenaceous or chitinous tests; the *Radiolaria* as Rhizopoda with differentiated sarcode-body, having central capsule, and very regular, radiated, silicious skeletons. A detailed account of the zoology and literature of each Order is given, and the subordinate groups are then treated *seriatim*. The arrangement of the Foraminifera is largely drawn from the labours of Messrs. Carpenter, Parker, and Jones, but it differs in two material points from any classification hitherto proposed, and to these it will be necessary briefly to allude.

In the arrangement proposed by the above-named English authors as well as in that of Prof. von Reuss, published about the same time, the Foraminifera were divided into two Sub-orders, *Imperforata* and *Perforata*, according to the condition of the test in respect to the minuter pseudopodial passages, and in so far no change is suggested. It has been customary hitherto to divide the *Imperforata* into three families characterised by "chitinous," "porcellanous," and "arenaceous" tests respectively. Dr. Zittel, after separating the chitinous forms, divides the remainder without reference to shell-structure, into two families—*Cornuspiridæ* and *Miliolidæ*, each of which contains both opaque-calcareous and sandy forms. The very names, as used to distinguish two large groups, are somewhat anomalous, as it may be clearly shown by the study of recent specimens, that the *Cornuspiræ* are only non-septate *Miliolæ*. Apart from nomenclature, there is perhaps

something to be said for this method of treatment, but the question is whether the difficulties it entails are not greater than those it is intended to avoid. The arenaceous and porcellanous types have one important character in common, in the imperforate condition of the test; and there is yet another peculiarity which some of the members of both groups possess not shared by the *Perforata*, namely, the tendency exhibited by such forms as tolerate brackish water (*Miliola* and *Trochammina*) to assume a more or less chitinous or membranous investment in proportion to the decreased amount of mineral constituents held in solution; whilst under similar conditions the shells of the more highly organised perforate forms (*Polystomella* and *Nonionina*) become thinner and more delicate, but never change their essentially calcareous nature. On the other hand, it can be easily shown that the *Globigerinida* have more points of connection with the arenaceous group than the *Miliolida* have, for whilst the latter furnishes but few examples with any approach to sandy shell-texture, the former has a number of types which might with good reason be associated with some which have hitherto been classed amongst the *Lituolida*, to form an intermediate group, calcareous and perforate under certain circumstances, arenaceous and imperforate under others.

The truth of the matter is that the variations of the Foraminifera are too multiform and the connection of the members of the Order one with another is too close to be well adapted for divisional classification, but they lend themselves readily and naturally to arrangement in linear series. Thus, in the first family of the English classification, MILIOLIDA, we find a large assemblage of forms of various degrees of complexity, but having, with trifling exception, compact, non-porous shells. In such forms as *Quinqueloculina agglutinans* and its fellows, the "porcellanous" overlaps the "arenaceous" series, rendering complete separation on the basis of shell-texture impossible. Next in order come a number of types essentially and invariably arenaceous—a series by no means uniform in the structure of the investment or even in the materials of which it is composed, but all composite, imperforate, and opaque. These give place to another intermediate set, partaking more or less of the characters of the "arenaceous" and the "perforate" groups, comprising such genera as *Endothyra*, *Valvulina*, *Textularia*, *Bulimina*, and the like, that are, it may be, clear-shelled and imperforate, sandy and imperforate, sandy externally but with a perforate shell as basis, or even hyaline and perforate, the mere size of the specimen having apparently much to do with the nature of the test. These supply any required number of transitional steps to the uniformly "perforate" types which constitute the highest group. We need not dwell further on this subject. To the systematist it is one of considerable difficulty, from whatever point it is viewed, and unless some better basis for classification than the minute structure of the shells of these little animals can be suggested, it may be a question whether an increase in the number of families by the recognition of an intermediate group, or possibly of more than one, would not be the course open to the fewest objections.

The second point that demands notice is the reconstruction of the important family NUMMULINIDA; for practically the characters assigned to it in the work before us would result in nothing less than reconstruction, if literally read. It is not needful to reprint the entire paragraph relating to the subject, for its essential element may be stated in few words, viz., the reliance on a complicated interseptal canal-system, as the characteristic feature of the Nummuline group. As one consequence of this limitation, and it is only one out of many that must ensue if consistently carried out, the genera *Amphistegina* and *Archædiscus* are placed amongst the *Globigerinida*. That there is some *primâ facie* ground for the change may be taken for granted, or it would not have found favour with so competent an observer as the author of the Handbook, but the more it is investigated the more we think it will appear that reliance on a single character of this sort is suited to the exigencies of an artificial system, rather than to the exposition of natural relationship. As the point in question is one of great importance, and involves the principles on which accepted methods of classification are based, it may be worth while to illustrate its general bearing by one or two instances of the results that would follow the adoption of a hard and fast definition of the nature proposed. Take for example the well-known genera *Polystomella* and *Nonionina*—types so closely related that the latter is often treated as a mere sub-genus of the former, and is perhaps best so regarded. In its higher modifications *Polystomella* has a very complicated canal-system, whilst no trace of such organisation has ever been traced in *Nonionina*. On the other hand, turning to the GLOBIGERINIDA, we find that *Rotalia* (proper) in its highest modifications has also a well-defined and complex canal-system, and the same, moreover, is easily recognised in *Calcarina*; so that this character, even according to Dr. Zittel's arrangement of genera, is not an exclusive feature of the NUMMULINIDA. Prof. W. K. Parker, than whom few have better right to be heard on such a point, regards *Amphistegina* (though a true generic type) as bearing a relation to *Nummulina*, similar in kind if not in degree to that which exists between *Nonionina* and *Polystomella*. It is true that neither in *Amphistegina* nor in *Archædiscus* has any true canal-system been demonstrated, but it must be recollected of the latter type that it has no septa, and it is possible that the double tubulation occasionally observable in its supplementary skeleton may represent this special organisation in a rudimentary condition. That *Polystomella* is a more highly organised type than *Rotalia*, and *Nummulina* presents a distinct advance upon either, and that in *general* terms the fact may be demonstrated by the relative complexity of the structure of the test, is hardly open to question. What is here contended for is this—that throughout the Foraminifera in each group comprising the modifications of a single central type, or of two or more closely allied types, there may be traced a regular series of subordinate forms gradually increasing in complexity of organisation, and that these cannot be separated in a system of classification without doing violence to the order of nature. In the types to which reference has already been made, such a sequence is easily found. In *Rotalia*, the minute thin-shelled, brackish-water *R. nitida* presents the very simplest morphological characters; *R. Beccarii* with its double septal walls marks a distinct advance, and, omitting a multitude of intermediates, *R. Schroeteriana* exhibits the highest development with a complete interseptal canal-system. In *Poly-*

stomella the most elementary variety of the type is found in the thin-shelled, simple *Nonionina depressula* of brackish-water pools, whilst *N. asterizans* and *Polystomella crispa* lead up to the complex *P. craticulata*, which is the parallel of the highest Rotalians. In like manner with *Nummulina*, though, as might be expected, the successive steps of differentiation are more distinct, and, as far as our present knowledge goes, further apart, it appears more consonant with analogy and more in accordance with natural order to regard *Archædiscus* and *Amphistegina* as closely related forms of inferior organisation leading up to the perfect type. The striking similarity in the general minute structure of the shell in these reputed Nummuline forms is confirmatory evidence not without value. The alterations rendered necessary by the adoption of the "presence of a canal-system" as the essential character of the family, could not stop where Dr. Zittel has left them; *Nonionina* and *Fusulina* would have to be transferred to the GLOBIGERINIDA, whilst *Calcarina*, *Tinoporus*, and some of the true *Rotaliæ* must under the restricted definition be severed from their natural allies to be placed amongst the NUMMULINIDA—changes that would find but little favour amongst students of the Rhizopoda.

There are many other little points in connection with the treatment of the Foraminifera that are open to criticism, favourable or otherwise, but as they do not affect the general usefulness and value of the work, it is needless to extend an already lengthy notice by their examination.

The RADIOLARIA, better known perhaps under Ehrenberg's name "Polycystina," form a much more manageable Order, and one which, in the present state of our knowledge, lends itself comparatively readily to artificial subdivision. The literature of the subject too is comparatively limited—that of the successive stages of investigation being summarised in the standard memoirs of Professors Ehrenberg, Johannes Müller, and Ernst Hæckel. The classification adopted by Prof. Zittel is with but little modification that elaborated by Prof. Hæckel for his magnificent monograph. The entire Order is divided into fourteen principal Groups, founded for the most part on the geometrical characters of the silicious skeleton. Out of the fourteen Groups, notwithstanding the enormous number of individuals and of species found in the early and middle Tertiary deposits of Barbados, Bermuda, North America, and the Mediterranean borders, only about one-half are known to have fossil representatives.

The Radiolaria make their appearance at a much later period of the earth's history than the Foraminifera and the part they have had to play in the formation of successive geological deposits has been a much less important one. Doubtful specimens have been found as far back as the Triassic beds of St. Cassian, but of too obscure a nature to yield satisfactory evidence as to geological range, and the same may be said of some that have been described of Jurassic age. In the Upper Chalk, however, well-defined and characteristic forms have recently been discovered by Dr. Zittel. In the earlier part of the Tertiary epoch the group assumes considerable importance, and from that time to the present Radiolaria have formed a frequent if not a constant element of the fauna of deep water.

The first part of the "Handbook" refers, in the main, to fossils belonging to one division of the Animal Kingdom, and it has therefore been necessary to dwell on points in which the mode of treatment differs from that which has hitherto prevailed, but the questions which have been adverted to in detail have a special and limited bearing, and do not materially affect the work in its wider aspect as a manual of palæontology. Of its excellence, when complete, as a student's text-book, and of its prospective value to the working palæontologist, the present instalment gives abundant promise.

There is but a word to add on the illustrative woodcuts. To those who recollect the beautiful drawings that accompany that section of the "Novara-reise," which is devoted to the Foraminifera of Kar Nikobar, the name of Dr. Schwager will be sufficient guarantee for accuracy and finish, and it is only needful to say that the draughtsman's hand has lost none of its cunning and that in the present work the illustrations, which are for the most part new, are singularly apt and effective, though, in the copy before us, occasionally somewhat marred by defective printing.

H. B. BRADY

OUR BOOK SHELF

Handbooks for the Glasgow Meeting of the British Association.—1. "Notes on the Fauna and Flora of the West of Scotland." 2. "Catalogue of the Western Scottish Fossils." 3. "Notices of some of the Principal Manufactures of the West of Scotland." (Glasgow: Blackie and Son, 1876.)

AS there are satisfactory guide-books to Glasgow and the West of Scotland already in existence, it would have been superfluous in the Local Committee to have compiled another general work of the same kind. It was, however, a happy idea to publish the three volumes which we have only now received, as they contain just such special information as cannot be readily obtained, but which it is to be supposed the many votaries of science who were recently assembled in Glasgow would be glad to be furnished with. The volumes are well printed, of a handy size, and, so far as we have been able to test them, carefully compiled by competent men. In the volume devoted to the fauna and flora, Mr. E. R. Alston describes the mammalia, Mr. Robert Gray the birds, Mr. Peter Cameron the insects, Mr. James Ramsay vascular flora, and Dr. J. Stirton the Cryptogamic flora. To vol. ii. is prefixed an Introduction by Prof. Young, on the geology and palæontology of the district, the catalogue itself being compiled by Messrs. James Armstrong, John Young, F.G.S., and David Robertson, F.G.S. This volume is illustrated with four plates of fossils. In the volume devoted to manufactures, Mr. St. John V. Day writes on the iron and steel industries, Mr. John Mayer on the engineering and ship-building industries, Mr. James Paton, Curator of the Glasgow Industrial Museum, on the textile industries, and Prof. John Ferguson on the chemical manufactures. Considering the haste with which these volumes must have been compiled, they are wonderfully complete and well arranged, and if the publishers are careful to keep them up to date and extend them in a new edition, they might become of permanent value. Prefixed to each volume is a sketch map of the country surrounding Glasgow, with its general geological features.

The Tree-lifter; or, A New Method of Transplanting Forest Trees. By Col. George Greenwood. Third Edition. (London: Longmans, Green, and Co., 1876.)

THIS is a book of some two hundred and thirty odd pages, eleven pages of which are devoted to a description of the

tree-lifter and of its advantages in transplanting large trees. The principle of transplanting trees with a large ball of earth attached to the roots is, however, so well known, and tree-lifters of similar construction to that here described are now so generally used, that we follow the example of the author in his brevity, and simply dismiss this part of the subject which he calls the "practical part of transplanting," and turn to Part 2, which is devoted to the "theory of transplanting, or physiology of trees in reference to transplanting." It is apparently for the purpose of recapitulating and condensing the views of various authors of acknowledged reputation in the several branches of vegetable physiology, and of expressing his own opinions thereon, that the author has put this book together, all that is really directly connected with the title being contained, as we have before said, in the first eleven pages. The author, however, at the beginning of Part 2, candidly says: "Before entering on physiology, I would say one word to defend myself from the charge of egotism and plagiarism. When I mention Sir Humphry Davy, I may say that immortal names are among those who have written on the physiology of trees; yet so much doubt and difference prevail among the authors on the subject, that one cannot adopt a single opinion without opposing many, held by minds, perhaps, as clear and comprehensive as Sir Humphry's. It is, then, to save the reader's time if I lay down as certain what men have doubted or controverted, or if I use the words, 'I think this,' or 'I think that,' in stating other people's opinion."

Space will not permit us to follow the author through all his reasonings; it will suffice to mention that no less than forty-one pages are given to the consideration of the subject of the course of the sap, in which the author tilts at several well-known English and Continental botanists whose theories are adverse to his own. Lindley is the most recent authority quoted in the original edition, and the opinions of later writers have not been embodied in subsequent issues. What we have already said will show the character of the book. The style of writing may be gathered from the first paragraph in the chapter on the course of the sap, where the author says: "However much we may dispute on *how* the sap gets into the tree, we shall all agree that it *does* get in somehow; and but for Dr. Lindley, I believe we should all agree on the *course* which it then takes." Further on, in connection with the theories of the contraction and expansion of the wood, caused by alternating heat and cold, and of the pumping action from the motion caused by wind, Col. Greenwood writes: "Look into the hot-house and the hot-bed. In these neither of these causes exists. Not a breath of wind enters; nor is any alternation of heat and cold allowed. Yet in these the ascent of the sap is freest. And if we look out of doors, I should say that the sap would be a slow traveller if its ascent depended on wind and cold. Here, then, I cannot back the favourite, and have a sort of blind leaning for *Turgescence*, or Swelling, a dark horse, certainly, and I am all in the dark about him myself;" and the author is in a similar state of gloom upon other points besides.

Practical Portrait Photography. By Wm. Heighway. (London: Piper and Carter, 1876.)

THE author of this little book of 152 pages has endeavoured to "provide simple and intelligible rules of working," as he states in his introduction, so that those who take up photography as an art should be helped over a number of difficulties certain to occur, and not always provided against in more ambitious works. The lessons chiefly enforced are cleanliness and accuracy in preparing the requisite solutions, and method in carrying out the rest of the processes; though these lessons may seem trite to the regular student of science, they are no doubt much needed amongst photographers who are not at the same time practised chemists.

The necessary instructions are well and carefully given, and the author has omitted no point of importance, taking the reader *seriatim* through every detail, from cleaning the glass plate to finishing the paper print.

We notice some errors in chemistry, where the author has given reasons for some of the processes, which we hope will be corrected in a future edition. R. J. F.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

On the Discovery of Palæolithic Implements of Inter-glacial Age

THE opinion that palæolithic man was a post-glacial being has been steadily losing ground among certain geologists whose studies render their opinions of considerable weight. Mr. Pengelly and Prof. Ramsay have stated their conviction that the old stone folk may have witnessed the commencement of glacial conditions, and have been driven south by the increasing severity of the climate. Prof. Dawkins has expressed his belief that while our rude ancestors hunted the elephant, glaciers still lingered in our mountain-valleys. Mr. Tiddeman goes further, and ascribes to them an inter-glacial age, and, as it seems to me, proved his point by the discovery of implements and a human bone beneath glacial-clay in the Settle Cave. Mr. James Geikie boldly advanced the opinion that *all* our palæolithic implements are of inter-glacial age, and an intimate knowledge of the glacial beds and gravels of the central and eastern counties led me independently to a similar conclusion. In making this last statement I particularly desire it to be understood that no claim is preferred to the theory as my own, for while I was almost fearful of my temerity in even thinking such things, my friend Mr. Geikie had brought his great stores of knowledge to bear upon the point, and has made it peculiarly his own. Nevertheless the fact that two geologists working independently in different districts should arrive at similar conclusions is no mean argument in our favour. The evidence upon which my convictions are based is given in my "Geology of the Fenland," and in my "Manufacture of Gun-flints," &c., to be shortly issued by the Geological Survey, and in the forthcoming edition of the "Great Ice Age."

Mr. Geikie has proved, and the work of Mr. Tiddeman, myself, and others, has confirmed the observations that the glacial epoch, instead of being an uninterrupted period of cold, was one of fluctuating climate, there being known at least four ice ages with intervening cold, mild, and warm epochs. The greatest severity of cold took place towards the early part of the Great Ice Age, and the great chalky boulder-clay which extends nearly to the Thames was then formed; no subsequent ice-sheet having left its traces further south than Lincolnshire. Travelling northwards from East Anglia we find this boulder-clay running under the purple boulder-clay, and this again overlaid still further north by a yet newer glacial bed. These are well-known facts accepted by all geologists, but as the old chalky boulder-clay has unfortunately been named the "upper" boulder clay, it has been supposed to mark the close of the glacial epoch, whereas it is only "upper" so far as East Anglia is concerned, and merely marks the last glaciation of that area, the more northern districts having been more than once glaciated since then. The East Anglian "upper" boulder-clay is probably as old as the Lancashire "lower" boulder-clay.

In consequence of this confusion of terms, the beds which overlie the chalky boulder-clay have been confidently relegated to post-glacial times, whereas all that can be determined by this superposition is that they are "post-boulder-clay." From valley and other gravels occupying such positions, great numbers of palæolithic implements have been obtained, especially from the basins of the rivers Lark and Little Ouse. Wherever bones are found in these gravels they belong to what Prof. Dawkins calls the pleistocene, and not to the pre-historic fauna; and this mammalian fauna is continued into the estuarine gravels of the Fenland, which contain extinct shells, such as *Cyrena fluminalis*, a shell which has often been found in gravels yielding palæo-

lithic, but never in beds yielding neolithic implements. As we travel northwards, say to Hessle, these *Cyrena*-bearing gravels are seen to underlie boulder-clay newer than the upper one of East Anglia; and in cave deposits still further north, beds yielding the same pleistocene mammals appear in similar situations, notably in the Victoria Cave at Settle. Now bearing in mind that in East Anglia, &c., where these beds are best developed, there has been no subsequent glaciation to sweep them bodily away, or show their age relatively to the glacial epoch, it seems to be a legitimate deduction that they are of inter-glacial age, when we find that to the north, wherever fragments have escaped destruction, they are overlaid by glacial beds. This is the conclusion to which Mr. Tiddeman arrived from a study of the Victoria Cave deposits and an intimate acquaintance with the glacial phenomena of the district, and my own work in the Fenland and East Anglia led me to a similar result. Mr. J. Geikie has, however, done more than any one to develop this idea, and was the first to propound it. He claims, then, that no palæolithic implement is of post-glacial date; and when we reflect upon the vast changes which have occurred since palæolithic times in the physical configuration of the country, in the mammalian fauna—changes which are even impressed upon the stable molluscs—the theory appears more than probable, and the difficulties which surround the post-glacial hypothesis steadily increase.

Palæolithic implements, however, are not all of one age (it would be strange indeed if they were), though it is very difficult to discriminate their relative antiquity. I have been much struck with the aged aspect of certain of the ruder tools as compared with some of the better finished ones and with the stones in the gravel in which they occur, and this gave me hopes of tracing such tools to an older deposit, a desire which has been abundantly fulfilled, and remarkably confirms my friend Mr. J. Geikie's bold surmise.

Here and there along some of the minor valley sides around Brandon are preserved patches of brick-earth, which are valuable as affording the only workable clay in the district. Whenever these beds are well exposed they are seen to underlie the chalky boulder-clay. Of this there cannot be the slightest doubt, for the glacial bed is typically developed and not in the slightest degree re-constructed. In these beds I have been so fortunate as to find palæolithic implements in two places; and in one of them quantities of broken bones and a few fresh-water shells. The implements are of the oval type, boldly chipped, but without any of the finer work which distinguishes the better made palæolithic implements. Although it would be rash to lay too great a stress upon the characters of these implements, it is, nevertheless, worthy of remark that they do belong to the crudest type. Equally rough specimens are found in the gravels above the boulder-clay and even among neolithic finds, still these very antique implements certainly do seem to belong to an earlier stage of civilisation if we regard them as examples of the best workmanship of their makers.

The interest attaching to these specimens lies in their exceeding antiquity—an antiquity greater than can be ascribed with certainty to any others. I have shown this boulder-clay to belong to the earlier part of the ice age, and beneath it these tools were found. I am not yet certain whether they belong to the so-called "middle glacial" series of Mr. Searles Wood, jun., to a somewhat newer date, or to a preceding period, for the beds lie directly upon the chalk. This much, however, is certain, that they conclusively prove man to have been a denizen of our land before the culmination of the glacial epoch.

Another point is deserving of notice. The tools are decidedly of palæolithic aspect—the difference between them and those which overlie the boulder-clay is slight in comparison with the differences between the latter and neolithic implements. Who shall say how long East Anglia was swathed in ice? Yet that interval was not long enough for man to advance greatly in his manufactures, and it appears to me we have here another argument in favour of the glacial age of all palæolithic tools and against the theory which relegates them to after the close of the ice age. It seems to bring the brick-earth tools and the gravel implements closer together, and withdraw them still further from the newer stone age.

As soon as the bones are examined and the survey of the brick-earths completed, I hope to write more fully upon this question, and here only indite a few preliminary notes in the hope that they may prove interesting to brother geologists.

SYDNEY B. J. SKERTCHLY

The Inverse Rotation of the Radiometer an Effect of Electricity

IN my communication published in NATURE, vol. xiv. p. 288, I endeavoured to show that the direct rotation of the radiometer was an effect of electricity. Before attempting to explain the inverse rotation it will be necessary to expound briefly some new facts which my electroscopic researches have led me to establish.

In order to ascertain the electric state of their inner surface, I exposed to solar radiation glass receivers such as are used for the air-pump. By means of the proof plane and electroscope I found that this surface was electrified negatively, and even to a greater degree than the exterior. This excess of energy I attribute to the numerous reflexions from the interior. If, however, we hold one of these electrified receivers near the Bohnenberger electroscope, taking care that it does not come in contact with it, the electroscope at once indicates the presence of *positive* electricity. As both the outer and inner surfaces are negatively electrified, this phenomenon must be attributed to the electricity developed in the interior of the glass itself by its molecular polarisation and feeble conductivity. The following experiment confirms this explanation. If we remove from the exterior by means of the proof-plane a portion of the negative electricity and then approach, as before, the globe to the electroscope, a remarkable increase of positive electricity is at once shown. The same results are observed in the radiometer.

I next examined the electric state of the exterior of the radiometer globe when placed in partial obscurity and moistened with ether. There are no signs whatever of electricity as long as the inverse rotation continues, but as soon as the direct rotation commences—on account of the obscure radiations given forth by the surrounding bodies—positive electricity manifests itself and rapidly increases. While in this state I exposed the radiometer to solar radiation, and I found that this positive electricity remains quite a long time, and that, notwithstanding the positive charge on the exterior, the direct rotation continues with its usual rapidity.

The fact last-mentioned enabled me to determine, by experiment, the electric state of the inner surface of the radiometer globe. Only two suppositions can be made in regard to it: either the electric state of the inner surface is dependent by means of molecular polarisation upon the electric state of the exterior, or it is independent. In the first supposition the interior face is electrified positively when the exterior is electrified negatively, and *vice versâ*. The second supposition may be divided into three hypotheses, for we can admit that the interior is constantly, under the same circumstances, either neutral, or negative, or positive. Hence we have in all four hypotheses, *à priori*, viz. :—

1. Inner surface is dependent upon electric state of exterior.
2. Inner surface is independent and neutral.
3. Inner surface is independent and negative.
4. Inner surface is independent and positive.

Now of these four hypotheses the fourth alone is verified by experiment. This I have established as follows :—

In one series of experiments I charged the exterior of the radiometer with positive electricity by exposing it to solar radiation.

In a second series I charged the same surface with positive electricity by exposing it to solar radiation after moistening it with ether.

Each experiment comprised two operations. I touched a certain number of times the exterior of the glass globe with the proof-plane and I carefully observed the electroscopic signs of the Bohnenberger electroscope when brought in contact with the proof-plane; then I approached to the electrometer the glass globe which had been partially discharged by the preceding experiment, and I again observed the signs given by the electroscope. In the case that one of the first two hypotheses expresses the real state of the inner surface of the radiometer under the influence of radiation, on approaching the glass globe we should have, in both series of experiments, electroscopic signs of equal intensity for equal electric charges of the exterior surface, manifested by the equality of those of the proof plane. Now this does not take place. In my experiments on the approach of the globe the electroscopic signs in the second series surpass in intensity those observed in the first series. These results agree perfectly with the fourth hypothesis, but are in open disaccord with the third. Any one can easily see this, with a little attention, by considering the layers of electricity produced in the interior of the glass walls by molecular polarisation. The fourth

hypothesis is, then, the true one, and the inner surface is electrified positively.

The explanation of both the direct and inverse rotation follows naturally from these facts and those communicated in my former note. For since the inner surface, when exposed to luminous or calorific radiations, is electrified positively, the direct rotation is a necessary consequence of the attractions and repulsions which this positive electricity exerts upon the free electricity of the vanes. This rotation continues when the radiometer is surrounded by light, because a perfectly homogeneous layer of electricity upon the inner surface is almost impossible.

The inverse rotation occurs in two circumstances—

1. When the instrument, having been exposed to radiation which produces a direct rotation is allowed to cool slowly.

2. When the radiometer at the ordinary temperature is cooled suddenly, for instance, by moistening it with ether.

In the first case, the electricity which the globe acquires when exposed to radiation disappearing very slowly, as experiments show, an inversion of the movement can be produced by an inversion in the signs of the electricity of the vanes. In fact, in accordance with the principle of reciprocity, the emission of the radiations gives rise in the vanes to a development of electricity equivalent and contrary to that which absorption has produced there. By this development of electricity the vanes would return to their neutral state if the electricity produced by absorption had not passed in part from the vanes into the rarefied gas of the globe. Now this passage took place with a greater energy as the rotary movement of the vanes had renewed more frequently the mass of air in contact with them. Hence the electric effect of the emission will be to change the signs and to diminish the charge of free electricity of the vanes.

In the second case, where the cooling is produced by moistening the exterior, the globe remains in its neutral state. For, as I have above remarked, during the whole time of the inverse rotation, the cooled surface of the globe gives no signs of electricity. It appears that the cooling itself is not capable of producing electricity, but that the passage of a radiation through the surface is absolutely required. In these conditions the vanes become charged with negative electricity upon the dark, and positive upon the bright side, by reason of the emission, at the same time that the radiations given forth by the vanes and absorbed by the inner surface of the glass globe electrify the latter positively.

Thus the electric theory of the radiometer explains quite well the principal phenomena which have been observed up to the present time. I hope to make, hereafter, a study of all the particular movements which different observers have noted in the accounts of their experiments. I will only say now that the most remarkable of them, viz., the rotation of the radiometer globe, when an obstacle is put to the rotation of the vanes, as discovered by Schuster, is in entire conformity with the above theory, while it constitutes a very serious objection to the hypothesis of mechanical impulsion by radiation.

JOSEPH DELSAULX, S.J.

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A Rudimentary Tail

A DAY or two ago a curious and interesting abnormality came under my notice, which, I think, deserves mention. I was examining the back of a girl, aged about eight, when I saw over the lower part of the sacrum, in the middle line of the back, a small hole, that, on the first glance, seemed like the opening of an old sinus. I was told, however that it had been present since birth, and I then looked at it more carefully. It had a direction downwards and somewhat forwards, and consisted of a reflection of the skin entering a more or less circular depression, about ¼ inch in diameter, and about ¼ inch deep. Not quite ¼ inch below its lower border could be felt the pointed extremity of the coccyx, which, instead of having its usual form, curved backwards and rather upwards. On stretching the skin downwards, that portion of it entering the depression or hole was raised, coming out like the top part of the finger of a glove which had been pressed down into the lower part, and a small prominence, about the height of the diameter of a pea, stood up from the surface; and this little sheath was found to cover and exactly fit the sharp end of the coccyx. The resemblance this bore to a rudimentary tail was sufficiently striking.

Jersey ANDREW DUNLOP

The Æolian Formation on the Lancashire Coast

IN the absence of large works on the subject, has your recent Waterloo correspondent seen the Survey memoir of the district around Southport in which the phenomena of wind driftage are treated in a brief yet quantitative manner? The efficient way in which pebbles and shells—as of *Mactra stultorum* (with which the shore is so plentifully covered)—especially when the convex side of a valve is presented vertically towards the direction of the storm winds, protect a small area to leeward, forming a miniature crag-and-tail arrangement, would seem to suggest that a solid screen offering an unbroken surface to the action of the wind, and at some distance from the region threatened, would be far more useful than the present expedients of growing marram grass, &c., to consolidate the dunes, or of planting lines of bare stakes. Practical men would easily devise a cheaply constructed barrier of old ship-timber faced with ling or other accessible material, or perhaps use the sand-hills themselves when armoured with tabular blocks of stone made on the spot by some such process as employed in the construction of the sea-walls of the Suez Canal. Land sold for building plots on exposed points ought surely to have some adequate defence against the devouring sand.

WILLIAM GEE

Manchester, Sept. 15

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE OF 1885, SEPT. 8–9.—The following elements, though approximate only, will suffice to give a pretty fair indication of the circumstances under which the totality of this eclipse will take place:—

Conjunction in R.A., 1885, Sept. 8, at 9h. 18m. 58s. G.M.T.

	°	′	″	
R.A.	167	25	39	
Moon's hourly motion in R.A. ...		34	36	
Sun's ,, ,, ...		2	15	
Moon's declination	4	30	44	N.
Sun's ,, ...	5	23	40	N.
Moon's hourly motion in declination ...		10	58	S.
Sun's ,, ,, ,, ...		0	57	S.
Moon's horizontal parallax		59	43	
Sun's ,, ,, ...		0	9	
Moon's true semi-diameter		16	16	
Sun's ,, ,, ...		15	54	

Hence the central and total eclipse begins upon the earth in long. 156° 54′ E., lat. 40° 54′ S., and ends in long. 75° 33′ W., lat. 74° 38′ S., and the sun is centrally eclipsed at apparent noon in long. 138° 39′ W., lat. 57° 40′ S.

The following are also points upon the central line:—

Long.		Lat.	Long.		Lat.
° ′		° ′	° ′		° ′
173 26 E.	...	40 28 S.	177 58 W.	...	41 23 S.
175 3 E.	...	40 34	171 59 W.	...	42 39 S.
177 33 E.	...	40 46			

The semi-diameter of the shadow in these longitudes is about 55′. It would therefore appear that observations are not likely to be made to any useful purpose, except in the southern part of the northern island of New Zealand, and here the sun will have no great elevation above the horizon. If we calculate from the above elements directly for Wellington, assuming the longitude of this place 11h. 39m. 20s. E., and its latitude 41° 17′, we find—

				h.	m.	s.		
Partial	eclipse	begins	Sept. 9 at	6	18	0	A.M.	Mean times at Wellington.
Total	,,	begins	,,	7	42	22	,,	
Total	,,	ends	,,	7	43	0	,,	
Partial	,,	ends	,,	8	58	0	,,	

And therefore the duration of totality 38 seconds only, with the sun at an altitude of 15°.

Calculating similarly for one of the points upon the central line, some fifty miles north of Wellington, or long. 175° 3′ E., lat. 40° 34′, the totality is found to commence at 7h. 41m. 31s. A.M., local mean time, and to continue 1m. 54s., with the sun at an altitude of 16°.

At Wellington the sun rises at 6h. 21m.

AN INTRA-MERCURIAL PLANET (?).—A second letter from Prof. Rudolph Wolf, of Zurich, giving further particulars relating to M. Weber's observations at Peckeloh, near Münster, on April 4, 1876, was communicated by M. Leverrier to the Paris Academy on the 11th inst. The sky had been cloudless up to noon, and neither spot nor *facula* was remarked, though the sun's disk was examined three or four times, according to M. Weber's custom. After noon the sky clouded until between 4^h and 5^h, when it cleared in places, and the sun was visible from twenty to twenty-five minutes. Utilising this interval, "M. Weber ne vit pas de facule, quoiqu'il eût promené la lunette sur toute la circonférence du soleil. Tout à coup un petit disque bien arrondi de 12 secondes d'arc se montra. Il se trouvait à 11 secondes de temps du bord oriental, et à la même distance au nord de l'equateur céleste (*sic*). L'astronome eut le temps d'examiner de très-près le voisinage de la tache, et nulle part il n'aperçut le plus imperceptible mouvement de facule, nulle part un nuage avoisinant. Seul le petit disque foncé se détachait sur le fond solaire."

The sky soon after clouded, and it was only at five o'clock on the following morning that it was possible to ascertain that "the phenomenon had disappeared from the surface of the sun." The Peckeloh observation was made at 4h. 25m. P.M., mean time at Berlin. It will be remarked that the observation leaves something to be desired as regards clearness.

The 1st, 2nd, and 3rd of next month are dates when it is desirable the sun's disk should be closely examined for any abnormal spot.

THE BRITISH ASSOCIATION

AMONG the later discussions of the meeting no doubt that which has excited most general notice was the debate on Prof. Barrett's paper "On Certain Abnormal Conditions of Mind." There can be little question that in one sense it dealt with subjects suitable for the department of Anthropology, and the scientific repute of Mr. Crookes, Mr. Wallace, Lord Rayleigh, and Prof. Barrett, necessitates the careful examination of anything they may bring forward. But it is doubtful whether the interests of science are best served by the introduction of subjects which are sure to provoke heated and unscientific discussion at a mixed meeting like that of the Association. Dr. McCann did not obtain very much favour for his ill-judged and extravagant scheme of endowed research which he propounded. A good suggestion was thrown out by one of the foreign visitors at the Lord Provost's spendid banquet to the principal members of the Association, in favour of close union and inter-communication between the British and similar Associations in other countries.

The General Committee passed the following resolution relative to the proposed museum of scientific instruments :—"That the Council be requested to take steps to urge upon her Majesty's Government the advisability of forming a museum of scientific instruments and chemical products, as suggested in the memorial of June last to the Lord President of the Council." The Committee also approved a recommendation that in future the presidents-elect of the various sections be invited to confer with the general secretaries, preparatory to the issue of the first number of the daily *Journal* at each meeting, to arrange the order in which the sectional addresses shall be delivered. Thus members may have an opportunity of hearing more than one sectional address.

The following is a list of the grants made at this meeting for scientific purposes ; the name prefixed is in each case that of the person entitled to call upon the treasurer for the amount :—

Mathematics and Physics.	£
*a*Everett, Prof.—Underground Temperature	50
*a*Stokes, Prof.—Reflective Powers of Silver and other Substances (renewed)	20
Thomson, Sir William.—Measurement of the Lunar Disturbance of Gravity	50
*a*Tait, Prof.—Thermoelectricity (renewed)...	50
*a*Cayley, Prof.—Publication of Tables of Elliptic Functions	250
*a*Joule, Dr.—Determination of the Mechanical Equivalent of Heat	100
*a*Glaisher, Mr. J.—Luminous Meteors	30
Forbes, Prof. G.—Observation of Atmospheric Electricity in India	15
Chemistry.	
*a*Allen, Mr.—Estimation of Potash and Phosphoric Acid..	20
Wallace, Dr. W.—Light from Coal Gas	20
*a*Clowes, Dr. F.—Action of Ethyl Bromo-Butyrate on Ethyl Sodaceto-acetate (renewed)	10
*a*Armstrong, Prof.—Isomeric Cresols and the Law of Substitution in the Phenol Series (renewed)	10
Hartley, Mr. W. N.—Double Compounds of Cobalt and Nickel	10
Brown, Prof. Crum.—Quantitative Estimation of Atmospheric Ozone	15
Hartley, W. N.—Liquid Carbonic Acid in Minerals ...	20
Geology.	
*a*Evans, Mr. J.—Kent's Cavern Exploration	100
*a*Lubbock, Sir J., Bart.—Exploration of Victoria Cave, Settle	100
*a*Evans, Mr. J.—Record of the Progress of Geology ...	100
*a*Hull, Prof.—Underground Waters in the New Red Sandstone and Permian	10
*a*Herschel, Prof.—Thermal Conductivities of Rocks ...	10
*a*Bryce, Dr.—Earthquakes in Scotland	10
Topley, Mr.—Sub-Wealden Exploration	100
Biology.	
Gamgee, Prof.—Physiological Action of Ortho-, Pyro-, and Metaphosphoric Acids	15
Hooker, Dr.—Report on the Family of the Diptero-Carpeæ	20
*a*Stainton, Mr.—Record of Zoological Literature	100
*a*Huxley, Prof.—Table at the Zoological Station at Naples	75
*a*Lane Fox, Col.—Exploration of Ancient Earthworks (renewed)	25
Lane Fox, Col.—Instructions for the Use of Travellers...	25
Statistics and Economic Science.	
*a*Farr, Dr.—Anthropometric Committee (partly renewed).	100
*a*Hubbard, Right Hon. J. G.—Common Measure of Value in Direct Taxation	10
Mechanics.	
*a*Froude, Mr. W.—Instruments for Measuring the Speed of Ships (partly renewed)	50
Thomson, Sir William.—Secular Experiments on the Elasticity of Wires	100
	£1,620

a Reappointed.

At the concluding general meeting Mr. Griffith read the list of grants, and stated that 2,731 tickets had been sold, producing 2,983*l*. In detail, there had been present 211 old life members, 31 new life members, 318 old annual members, 208 new annual members, 1,243 associates, 696 ladies, and 24 foreign members. Sir John Hawkshaw moved a general vote of thanks to the local authorities and officials, especially mentioning Lord Provost Bain, Sir James Watson, Mr. Grahame, Dr. Blackie, and Mr. J. R. Napier. He said that the Lord Provost's kindness and geniality of disposition, his intelligence, and his power of unlimited work, were most remarkable. Capt. Galton, in seconding the motion, said he had never come in contact with a more energetic local committee.

The hospitality displayed at some of the excursions was magnificent, and the foreign visitors had been most cordially received. Prof. Stokes, of Cambridge, proposed thanks to the University of Glasgow for the very great accommodation it had afforded to the Association ; the motion was seconded by Dr. Carpenter. Sir William Thomson proposed the vote of thanks to the President. He thought Dr. Andrews's presidency would be beneficial to the Association in many ways. In his address there were many things the serious and permanent consideration of which would prove most beneficial to the progress of science and of higher education in the country. Dr. Allen Thomson, the President-Designate for 1877, seconded the motion. Dr. Andrews, in responding, expressed his gratification at the scientific character of the meeting, which, he thought, would bear comparison with any other. All the sections had been above the average, and in Section A. numerous papers of no ordinary importance were read. He referred especially to a paper by Dr. Ker, of Glasgow, who had followed up one of the most difficult researches of Faraday, and had presented a paper of great originality and extreme value. There had been little that was sensational in their proceedings, but he believed even the public at large would greatly prefer true scientific work to excitement.

This meeting has been notable for the attendance of eight ex-presidents, viz., Prof. Stokes, Dr. Carpenter, Sir William Thomson, Prof. A. W. Williamson, Sir John Hawkshaw, Dr. Hooker, Dr. Lloyd, and the Duke of Argyll.

REPORTS.

Report of the Committee for Testing Experimentally the Exactness of Ohm's Law, drawn up by Prof. Clerk Maxwell.—The statement of Ohm's law is, that for a conductor in a given state the electromotive force is proportional to the current produced.

If we divide the numerical value of the electromotive force by the numerical value of the current, the quotient is defined as the resistance of the conductor, and Ohm's law asserts that the resistance, as thus defined, does not vary with the strength of the current. The difficulty of testing this law arises from the fact that the current generates heats and alters the temperature of the conductor, so that it is extremely difficult to ensure that the conductor is at the same temperature when currents of different strength are passed through it.

Since the resistance of a conductor is the same in whichever direction the current passes through it, the resistance, if it is not constant, must depend upon even powers of the intensity of the current through each element of the conductor. Hence, if we can cause a current to pass in succession through two conductors of different sections, the deviations from Ohm's law will be greater in the conductor of smaller section, and if the resistances of the conductors are equal for small currents they will be no longer equal for large currents.

The first method which occurred to the Committee was to prepare a set of five resistance-coils, of such a kind that their resistance could be very accurately measured. Mr. Hockin, who has had great experience in measuring resistances, suggested 30 ohms as a convenient magnitude of the resistance to be measured. The five coils, and two others to complete the bridge, were therefore constructed, each of 30 ohms, by Messrs. Warden, Muirhead, and Clark, and it was found that a difference of one in four millions in the ratio of the resistance of two such coils could be detected.

According to Ohm's law, the resistance of a system consisting of four equal resistances joined in two series of two, should be equal to that of any one of the coils. The current in the single coil is, however, of double the intensity of the current in any one of the four coils. Hence, if Ohm's law is not true, and if the five coils when compared in pairs with the same current are found to have equal resistances, the resistance of the four coils combined would no longer be equal to that of a single coil.

A system of mercury cups was arranged so that when the system of five coils was placed with its electrodes in the cups, any one of the coils might be compared with the other four combined two and two.

After this comparison had been made, the system of five coils was moved forward a fifth of a revolution, so as to compare the second coil with a combination of the other four, and so on.

The experiments were conducted in the Cavendish Laboratory by Mr. G. Chrystal, B.A., Fellow of Corpus Christi College, who has prepared a report on the experiments and their results.

A very small apparent deviation from Ohm's law was observed, but as this result was not confirmed by the much more searching method of experiment afterwards adopted, it must be regarded as the result of some irregularity in the conducting power of the connections.

The defect of this method of experiment is that it is impossible to pass a current of great intensity through a conductor without heating it so rapidly, that there is no time to make an observation before its resistance has been considerably increased by the rise of temperature.

A second method was therefore adopted in which the resistance was compared by means of strong and weak currents, which were passed alternately through the wires many times in a second. The resistances to be compared were those of a very fine and short wire inclosed in a glass tube, and a long thick wire of nearly the same resistance. When the same current was passed through both wires, its intensity was many times greater in the thin wire than in the thick wire, so that the deviation, if any, from Ohm's law, would be much greater in the thin wire than in the thick one.

Hence if these two wires are combined with two equal large resistances in Wheatstone's bridge, the condition of equilibrium for the galvanometer will be different for weak currents and for strong ones. But since a strong current heats the fine wire much more than the thick wire, the law of Ohm could not be tested by any ordinary observation, first with a weak current and then with a strong one, for before the galvanometer could give an indication, the thin wire would be heated to an unknown extent. In the experiment, therefore, the weak and strong current were made to alternate thirty and sometimes sixty times in a second, so that the temperature of the wire could not sensibly alter during the interval between one current and the next.

If the galvanometer was observed to be in equilibrium, then if Ohm's law is true, this must be because no current passes through the galvanometer, derived either from the strong current or the weak one. But if Ohm's law is not true, the apparent equilibrium of the galvanometer needle must arise from a succession of alternate currents through its coil, these being in one direction when the strong current is flowing, and in the opposite direction when the weak current is flowing. To ascertain whether this is the case we have only to reverse the direction of the weak current. This will cause the derived currents through the galvanometer coil to flow both in the same direction, and the galvanometer will be deflected if Ohm's law is not true.

Mr. Chrystal has drawn up a report of this second experiment, giving an account of the mode in which the various difficulties were surmounted. Currents were employed which were sometimes so powerful as to heat the fine wire to redness, but though the difficulty of obtaining a steady action of the apparatus was much greater with these intense currents, no evidence of a deviation from Ohm's law was obtained, for in every experiment in which the action was steady, the reversal of the weaker current gave no result. The methods of estimating the absolute value of the currents are described in the report.

A third form of experiment, in which an induction coil was employed, is also described, but though this experiment led to some very interesting results, the second experiment gives the most searching test of the accuracy of Ohm's law.

Mr. Chrystal has put his result in the following form :—If a conductor of iron, platinum, or German silver of one square centimetre in section has a resistance of one ohm for infinitely small currents, its resistance when acted on by an electromotive force of one volt (provided its temperature is kept the same) is not altered by so much as the millionth of a millionth part.

It is seldom, if ever, that so searching a test has been applied to a law which was originally established by experiment, and which must still be considered a purely empirical law, as it has not hitherto been deduced from the fundamental principles of dynamics. But the mode in which it has borne this test not only warrants our entire reliance on its accuracy within the limits of ordinary experimental work, but encourages us to believe that the simplicity of an empirical law may sometimes be an argument for its exactness, even when we are not able to show that the law is a consequence of elementary dynamical principles.

Abstract of the Twelfth Report of the Committee for Exploring Kent's Cavern, Devonshire. Read at Glasgow, September 8.—The Eleventh Report, presented by the Committee

to the Association, during the meeting at Bristol in 1875, brought up the narrative of the exploration to the end of July of that year. From that date the work has been carried on uninterruptedly in all respects as in previous years; and it is intended in the present report to describe the researches made during the thirteen months ending Aug. 31 of the present year.

The superintendents have had the pleasure, as in former years, of conducting a large number of persons into the cavern, of explaining to them on the spot the mode of working, and describing the facts which have been discovered, as well as of setting forth their bearing on palæontology and anthropology. The cavern has also been visited by numerous persons, who have been conducted by the "Guide," *i.e.* the foreman of the work, under arrangements laid down by the superintendents.

The Great Oven.—Your Committee stated last year, that on July 27, 1875, they began the exploration of the small passage known as "The Great Oven," which connects with one another "The Cave of Inscriptions" and "The Bear's Den"—the two remotest chambers of the cavern. The Great Oven may be said to consist of three reaches—the eastern, central, and western. The western reach—the only one which has been explored—extends tortuously from its commencement in the south-west corner of the Cave of Inscriptions, for a distance of 58 feet, where it is succeeded by the central reach. At its mouth it is 8 feet high, from the limestone roof to the bottom of the usual 4-feet excavations made by the Committee. Its width is commonly about 4 feet, but at one point it contracts to 3 feet, and at another expands to 7 feet. Throughout its entire length the roof and walls have the aspect of a well-worn water-course.

There was no continuous floor of stalagmite, though here and there portions of such a floor, perhaps never continuous, adhered to and projected from the walls; and pieces of stalagmite, as well as detached "paps" of the same material occurred in the deposit below. There was no reason to suppose that earlier explorers had ever worked in this branch of the cavern.

The deposits were a thin layer of "cave-earth," lying immediately on "breccia," without any intermediate crystalline stalagmite such as occurs in typical sections. At the entrance, and up to 34 feet from it, the usual 4-feet sections failed to reach the bottom of the breccia, so that its depth is undetermined; but at the point just mentioned, the limestone floor was found at a depth of 3·5 feet below the upper surface of the cave-earth, and thence to the inner end of the reach the floor was found everywhere at a depth of from 2 to 4 feet, thus displaying a continuous limestone floor for a length of 24 feet, and giving a pretty uniform height of 8·5 feet to this portion of the reach. The upper surface of the cave-earth ascended from the mouth to the inner end of the reach, at a mean gradient of about 1 in 7, whilst the limestone floor was inclined in the same direction at a somewhat higher gradient.

The total number of "finds" in this part of the Great Oven was 50. The remains yielded by the cave-earth included 2 teeth of hyæna, 6 of bear, 10 of ox, 1 plate of a small molar of mammoth, several bones and pieces of bone, including an astragalous of horse, a few coprolites of hyæna, a portion of a flint flake, and a flint chip.

The flake (No. 6672) is of a pretty uniform cream colour, almost a parallelogram in outline, 1·4 inch long, ·7 inch broad, abruptly truncated at each end—one of which retains the original surface of the nodule from which it was struck—and 3 inches in greatest thickness. The inner surface is slightly concave, whilst the outer is very convex, and consists of three planes or facets, the central one commencing near the but end, whilst those on each side of it extend the entire length of the flake. Its ridges, and, excepting a very few small notches, its lateral edges are quite sharp, and show that it can have had little or no wear and tear in any way, and that in all probability it reached the spot in which it was found, not by the transporting action of water, but by human agency. It was met with less than a foot below the surface of the cave-earth, 40 feet from the mouth of the Great Oven, on Oct. 13, 1875.

The specimens yielded by the breccia were ten teeth of bear and a few bones, none of which call for special description.

The exploration of the western reach of the Great Oven was completed on October 27, 1875, three months having been spent on it.

The Labyrinth.—The existence of the chamber termed "The Labyrinth" was probably known to but few persons when Mr. MacEnery commenced his researches in the cavern in 1825, as what appeared to be its *two* entrances must have then been so nearly filled as to reduce them to the size of mere pigeon-holes. These entrances are respectively about 190 and 200 feet from the mouth of what is called "The Long Arcade," from which the nearest external entrance of the cavern is about ninety feet farther. The name of *Labyrinth* was given to the branch of the cavern now under notice on account of the difficulty which, without a guide, visitors experienced in threading their way between the numerous masses of fallen limestone and the large bosses of stalagmite which occupied its floor. "There was," says Mr. MacEnery, "a tradition of the loss of life here by a young man who ventured to explore it without a guide. It is certain that two gentlemen, who lost their light and way, spent a night of horror here. Dreading to advance for fear of falling into the pits, they remained immovable until their friends came to their relief."[1]

The Labyrinth extends from the Long Arcade, in a south-easterly direction, for about forty-six feet, throwing off three narrow branches at and near its inner end. Of these the central one, opening out of the south-eastern corner, and which it is proposed to call "Matthews's Passage," after one of the workmen, leads into The Bear's Den.

The walls and roof of the Labyrinth, though by no means without traces of the erosive action of flowing water, are in most places extremely rugged, and suggest, by their fretted aspect, that even the last of the numerous blocks of limestone encumbering the floor must have fallen a long time ago.

It is separated from the Long Arcade by a massive curtain of limestone depending from the roof to the depth of nine feet, across a space about eighteen feet wide, being, so to speak, slightly looped up at each end to form two small entrances.

Mr. MacEnery had conducted some diggings in the Labyrinth, and had carried them to a depth of at least three feet at one of the entrances, so that, by assuming a stooping posture, ingress and egress became possible. In all other parts of the chamber his work was much less deep.

Omitting the large blocks of limestone, the deposits were:—

First, or uppermost, a floor of granular stalagmite, from which arose several large bosses also of stalagmite, one of which was eleven feet high above the floor, whilst its base occupied a circular space fully fifteen feet in mean diameter.

Second, a layer of cave-earth, rarely amounting to more than a foot in depth, and sometimes to not more than a few inches, whilst it occasionally reached as much as two feet.

Third, though it may be doubted whether there was a floor of the more ancient, the crystalline, stalagmite in the Labyrinth, the lower, and by far the greater part of the bosses mentioned above was of that variety, and was covered with a comparatively thin envelope of the granular kind, without any mechanical deposit between them.

Fourth, the breccia, or, so far as is known, the most ancient deposit in the cavern, lay immediately beneath the cave-earth, from which there was nothing to separate it, and extended to a depth exceeding that to which the excavations were carried.

In order to achieve the thorough exploration of the Labyrinth, it was necessary to break up all the bosses of stalagmite, with the exception of the largest of them, of which a portion has been left intact, it being believed that it shows strikingly the utter inadequacy of the data derived from a *boss* to solve the problem of the amount of time represented by a *floor*, and *vice versâ*.

The upper surface of the cave-earth rose from the mouth of the Labyrinth to its innermost extremity at a mean gradient of about 1 in 17.

The total number of "finds" in this branch of the cavern was 135, and the specimens they included were as follow:—

Lying on the Surface.—Three portions of ribs and two other bones, the two latter having been cut with a sharp tool, perhaps by an existing butcher, and one bone of bat.

In the Granular Stalagmite.—1 tooth of lion.

In the Cave-earth.—32 teeth of hyæna, 7 of bear, 6 of fox, 3 of horse, 2 of rhinoceros, 3 plates of a molar of a young mammoth, 1 of lion, 1 of ox, and 1 of sheep (of doubtful position); several bones and portions of bone, including a tarsus of bird, and two pieces of bone apparently charred; 1 coprolite, and 1 small chip of flint.

In the Crystalline Stalagmite.—6 teeth of bear, of which 5 were in one and the same jaw.

In the Breccia.—215 teeth of bear, and a considerable number of bones, of which many are good specimens.

The exploration of the Labyrinth was commenced on October

[1] See "*Trans.* Devon. Assoc.," vol. iii., p. 238.

28, 1875, and completed on July 10, 1876, upwards of eight months having been spent on it.

Matthews's Passage.—Having finished their researches in the Labyrinth, the Committee proceeded at once to explore the small branch leading from it to the Bear's Den, and termed, as already stated, Matthews's Passage, thus leaving the two other and adjacent small ramifications to be undertaken on some future occasion. To this course they were tempted mainly by the wealth of osseous remains which, from Mr. MacEnery's description, they are likely to find in the Bear's Den.

Matthews's Passage consists of two reaches. The first extends for about 14 feet towards the south-east, where the second turns sharply towards east-north-east, and, after a somewhat tortuous course of about 15 feet, enters the Bear's Den. Their height is from 9 to 10 feet almost everywhere, measuring, as usual, from the bottom of the excavation, which nowhere reaches the limestone floor; and they vary from 3·5 to 7 feet in width. The walls and roof, the latter especially, bear evident traces of the erosive action of a flowing stream, succeeded by the corrosion due, no doubt, to acidulated water, as the surfaces are much fretted.

There were but scanty traces of a stalagmitic floor in the first reach, in which, however, the earlier explorers had here and there broken ground; but throughout the entire length of the second reach a floor extended from wall to wall, varying from 10 to 24 inches in thickness.

The mechanical deposits in the first reach were the usual thin layer of cave-earth above, and the breccia of unknown depth below; but in the second reach the space beneath the stalagmitic floor was mainly occupied with large loose masses of limestone, some of which required to be blasted more than once in order to remove them. The spaces between them were filled with cave-earth or breccia, with comparatively few specimens of any kind.

The upper surface of the cave-earth was almost perfectly horizontal in the first reach; but in the second it rose towards the Bear's Den at a gradient of about 1 in 7.

Matthews's Passage yielded a total of 49 "finds," including specimens which may be thus distributed:—

In the Cave-earth.—26 teeth of hyæna, 2 of bear, 1 of an immature mammoth, 1 of fox, and a considerable number of bones, many of them being broken, and a few of them gnawed.

In the Breccia.—100 teeth of bear, and a large number of bones. The richest "finds" were met with in a small recess at the junction of the two reaches, where the teeth and bones were huddled confusedly together, suggesting that a rush of water had probably carried them to the spot they occupied.

No trace of man was detected in any part of the Passage, the exploration of which was completed on August 31, 1876, having occupied about seven weeks.

In looking over the work accomplished since the Eleventh Report was presented in 1875, the following noteworthy facts present themselves:—

1. In their Eleventh Report, the Committee sketched the distribution, in the cavern, of the remains of the mammals which characterise the cave-earth. Of this sketch, the following is a brief summary:—The hyæna had been met with wherever the cave-earth was found; the hare had not been detected anywhere in the western division of the cavern—that most remote from the external entrances; the badger, wolf, and ox had not been found beyond "The Charcoal Cave;" and relics of horse, rhinoceros, deer, fox, elephant, and lion had not appeared beyond "The Long Arcade."

It is now necessary to say that remains of ox, horse, rhinoceros, fox, elephant, and lion have all been found *beyond* the Long Arcade, in one or more of the three branches of the cavern explored since the Bristol meeting. In all other particulars the distribution remains at present as sketched in 1875.

2. No tooth, or, so far as is at present known, other trace of *Machairodus latideus* has been met with since the last Report was drawn. In short, the only evidence of the presence of this mammal which the Committee have detected during the continuous labour of almost twelve years, is the solitary incisor found July 29, 1872, a fact well calculated to impress one with the unsatisfactory nature of merely negative evidence. It cannot be doubted that had this comparatively small specimen been overlooked, those palæontologists who were sceptical respecting the occurrence of Machairodus in Kent's Hole, would have believed their scepticism to be strongly confirmed by the labours of the Committee, whilst the number of sceptics would have been greatly increased.

3. As already stated, the Committee spent upwards of ten consecutive months, in 1875–76, in exploring the Labyrinth and Matthews's Passage; yet, during all this time, and in these two important branches of the cavern, they found no trace whatever of prehistoric man. Had they, on receiving their appointment from the British Association in 1864, commenced their researches in either of the branches just named—and such a course was by no means without its advocates—instead of beginning at the external mouth of the cavern, and proceeding thence steadily through the successive chambers and galleries, there can be little or no doubt that Kent's Hole would have been pronounced utterly destitute of any evidence on the question of human antiquity, and but poorly furnished with remains of extinct mammalia. The work would probably have been closed without going further, to the great loss of anthropology and palæontology, as well as of popular education in these important branches of science.

Seventh Report on Earthquakes in Scotland, by Dr. James Bryce, F.G.S.—The past year was a period of comparative quiescence in Scotland. Dr. Bryce described the arrangements made for recording future shocks in the Comrie district. The Committee recommended the erection of seismometers at Ardoch, Dunblane, and Bridge of Allan, where very distinct disturbances were felt in 1873.

Second Report of the Committee on Underground Waters of the New Red and Permian Formations of England, by C. E. de Rance, F.G.S.—The Committee's inquiries have been continued last year, particularly with reference to Liverpool, Birkenhead, Nottingham, and Birmingham. Information has also been promised from Staffordshire. The Committee hope to complete their labours before next meeting of the Association.

Statistics were given by Mr. de Rance regarding the amount of water obtained from wells at Liverpool, Coventry, Birmingham, Leamington, Nottingham, Birkenhead, Warrington, and Stockport. It was mentioned that at Liverpool the level of the water in the public wells is gradually being lowered.

At Barrow-in-Furness a bore for coal 3,210 feet deep, struck, at the depth of 250 feet, a spring which now yields 13,500 gallons daily, and rises 12 feet above the surface. In this case, as had been predicted by Mr. Aveline, a member of the Committee, the Permian rocks were found directly overlying the Millstone Grit, and it was thus proved that the Coal Measures lying to the north are not continuous beneath the Permian. Another important circumstance discovered by this bore was the existence of petroleum in the Millstone Grit.

The New Red Sandstone, being porous and ferruginous, has been found to filter the water and oxidise the organic matter contained in it. Water from wells in the New Red, even when not artificially filtered, ranks high among drinking-waters for purity and wholesomeness, containing little saline and hardly any organic matter.

Taking an average rainfall of 30 inches per annum, and granting that only 10 inches percolate into the rock, the supply of water stored up by the Permian and New Red formations was estimated by the Committee to amount to 140,800,000 gallons per square mile. This rate would give, for the 10,000 square miles covered by the formations, 1,408,000,000,000 gallons. Only a very small proportion of this amount is made available for the supply of cities and towns.

Report on Lower Bagshot Leaf and Fruit Beds, by Mr. W. S. Mitchell.

SECTION B.—Chemical Science.

In Section B the amount of work done during the meeting was very considerable, and the quality of the work was fairly good. On Thursday a considerable number of members attended to listen to the president's address, which has been already reported. The papers read on that day were not of any great interest.

Mr. Pattison Muir gave an account of some preliminary investigations upon *Essential Oil of Sage*. Mr. A. R. Newlands read a paper calling attention to various relations which exist among the atomic weights of the Elements. The greater part of the matter contained in this paper has been, at various times, already made public by Mr. Newlands. In a paper by Mr. J. J. Coleman upon *A New Condensing Machine for the Liquefaction of Gases by combined Cold and Pressure*, attention was drawn to certain dynamical questions relating to the best method of obtaining cold from compressed gas so as to utilise the cold produced

in expansion. Mr. Coleman's paper could not well be understood without the sketch which accompanied it. A lengthy paper by Mr. W. Ramsay followed, upon *Picoline*. The author described many new salts of picoline, especially those formed by the action of the halogens, which he showed might be classed as—

1. Picoline + 2 atoms of halogen.
2. Picoline + 1 molecule of haloid acid.
3. Picoline + 1 molecule of haloid acid + 2 atoms of halogen.

By the action of chlorine on picoline an oily body may be also produced, from which, by the addition of water, a solid is obtained, which is probably a hypochlorite derivative. Various other salts of picoline were described. The author thought that discussions concerning the constitutional formula of picoline were as yet premature; his investigations, however, appear to show that this base is not a nitrile nor carbamine, and that it does not contain the methyl group. On oxidation it yields Dewar's pyridene dicarbonic acid.

The last paper read was by Mr. J. Stoddard, *On the Zinc Desilverising Process*. It was of purely technical interest.

On Friday the Section had its hands full of sewage, the result, as might have been anticipated, being unsatisfactory. The papers read on the sewage question were:—*Report of Committee; Experimental Researches on the Chemical Treatment of Town Excreta*, by Mr. J. Coleman; and *Sewage Purification and Utilisation*, by Mr. J. Banks. The committee's report was confined to operations conducted at Romford Farm on irrigation. During the time of experiment it appeared that the nitrogen retained by the crops amounted to 30·34 per cent. of that received in the sewage; the yield of rye grass was good. The committee did not ask meanwhile to be reappointed. Mr. Coleman advocated the use of charcoal, large quantities of which might be obtained in the form of the residue removed from the retorts in the distillation of shale oil. Mr Banks recommended filtration through large beds of wood or peat charcoal, placed in wire cages, with subsequent aëration by exposing the sewage in the form of a thin cascade, to the action of the atmosphere. In the discussion it was admitted that the operations at Romford were carried on at a loss; Mr. Allen congratulated the advocates of irrigation on their acknowledgment of this fact, saying that the sooner they got rid of the idea of making this matter pay, the better. Dr. Fergus traced all the woes of humanity to the water system now in vogue in large towns; Mr. Spence believed in precipitation, while Dr. Gilbert manfully upheld irrigation and filtration.

As usual, when dealing with sewage, everyone held by his own opinion, and no two people agreed as to what was to be done.

In Mr. Allen's report of the work of the committee appointed to investigate the accuracy of the various methods adopted for analysing "Commercial Phosphates and Potash Salts," the latter part of the problem was alone dealt with. The committee approved of Tatlock's method somewhat modified; that is, they thought that soda salts are best removed by washing with a strong solution of platinic chloride, followed by washing with alcohol; but they recommended that in the presence of much sulphates, the method should be modified by getting rid of the greater part of such sulphates by means of barium chloride before adding platinic chloride. Mr. Allen, who read the report, personally did not approve of the plan of adding sodium chloride in order to convert the potassium sulphate into chloride, because in the presence of large quantities of soda salts he always found the results come out rather low; washing with platinic chloride appearing under these circumstances to remove, along with the soda salts, a portion of potassium salt likewise.

In a short paper *On the Physiological Action of Pyro- Meta- and Ortho-Phosphoric Acids*, Dr. Gamgee, F.R.S., showed that while the ortho acid is physiologically inert, the pyro acid is very poisonous, and the meta acid is intermediate in its action.

A paper by Mr. F. H. T. Allan, *On a Safe and Rapid Evaporating Pan*, concluded the day's proceedings.

On Monday morning the Section was summoned to hear Prof. Thorpe's *Report on the Specific Volumes of Liquids*, but owing to the absence of the author the paper was taken as read.

The committee appointed for the purpose of collecting and suggesting subjects for chemical researches, after obtaining the opinions of various well-known chemists, did not recommend a continuation of their labours.

A number of papers were then read. Dr. Emerson Reynolds described experiments on the specific heat of beryllium, which went to prove that the atomic weight of that metal is 9·2; the atomic heat deduced from Dr. Reynolds's experiments being, on this assumption, equal to 5·91. Incidentally Dr. Reynolds showed that the modification of Bunsen's calorimeter used by him might be employed in class experiments, and the accuracy of the law of Dulong and Petit in certain instances thereby demonstrated to students.

Mr. Johnstone Stoney, F.R.S., amused and interested the Section by a number of drawings of tetrahedra, octahedra, &c., on to which he dexterously stuck representations of oxygen atoms, chlorine atoms, and so on. His general endeavour seemed to be to convince his auditors that in most basic salts oxygen is divalent, being in direct combination with the acidifying constituent of the molecule, but that when oxygen is not so directly related to this constituent in basic salts, it is tetravalent.

Dr. Macvicar, of Moffat, brought forward some of his peculiar views as to the constitution of matter, in a paper entitled *On the Possible Genesis of the Chemical Elements out of a Homogeneous Cosmic Gas or Common Vapour of Matter*.

Mr. E. H. Biggs described a new form of voltaic battery. The positive pole consists of a perforated carbon plate, which divides the jar into two divisions; the perforations are closed by means of earthenware plugs. The negative pole consists of a zinc plate. Dilute sulphuric acid is poured into the zinc compartment, and a good oxidising agent into the other. The current is intense, and the result a good constant battery.

The president described a few new derivations of anthracene, remarkable for their instability. Mr. J. T. Brown communicated a note *On Anthracene-testing*.

A modification of the sodium sulphide process for the manufacture of soda ash was described by Mr. W. Welden, under the title of *A Means of Suppressing Alkali Waste*. The sodic sulphate and carbonaceous matter are separately heated, and then brought into contact in a furnace lined with carbon. The sulphuretted hydrogen evolved in the conversion of the sodic sulphide into carbonate is conducted into water containing very finely divided oxide of iron or of manganese; the metallic sulphide so produced is subjected to the action of air, whereby sulphur is thrown down; fresh quantities of sulphuretted hydrogen are then passed in, aëration is again carried out, and so on until about 85 per cent. of sulphur to 15 per cent. of metallic oxide is present. This mixture is dried, and used in the manufacture of sulphuric acid.

Dr. C. R. A. Bright gave a description of some new derivatives of cotarnine, and Mr. Kingzett described briefly his later researches on the *Oxidation of Terpenes*: he stated that the liquid obtained by the oxidation of turpentine was possessed of marked antiseptic properties, which were to be traced to the presence of camphoric acid and peroxide of hydrogen in the liquid.

So many papers relating to technical chemistry were brought forward on Tuesday that it was thought better to sub-divide the Section, allotting the more purely scientific subjects to a sub-section. In this sub-section Dr. Letts described experiments which gave some countenance to the idea that a hydrocarbon having the formula $C_{10}H_{17}$ really existed. His experiments, were not, however, of so exact a nature as to carry conviction to the minds of many of the members. Mr. J. Buchanan described a modified hydrometer used on board the *Challenger*, and also an instrument for registering pressure and temperature at considerable depths.

Papers were read by Dr. Gladstone *On the Copper Zinc Couple*, and by Mr. W. N. Hartley *On Liquid Carbonic Acid in Minerals*.

Mr. R. Da Silva described the general action of hydriodic acid on mixed ethers, having the formula $C_nH_{2n+1}OCH_3$, and Dr. Cameron called attention to "Ammonic Selenio-cyanide." Of those papers which dealt with applied chemistry, the most interesting was one by Mr. J. A. R. Newlands, in which he described the *Alum Process in Sugar Refining*. The object of this process is to remove potash salts by the addition of ammonium sulphate in quantity sufficient to form alum, which is precipitated. The residual acid liquors are neutralised by means of lime. The other technical papers were chiefly occupied with sketches of the various chemical industries of Glasgow and the neighbourhood. Mr. F. Ward described a method for preparing the paper used for cheques, which prevents fraudulent alterations being made in the writing of the cheques.

On Wednesday morning the section met for a short time, when Mr. Pattison Muir read two papers *On Bismuth Compounds*, and *On the Action of Dilute Saline Solution upon Lead*.—Prof. Dewar described some experiments by which he has been able

to transform chinoline into aniline. Chinoline, or more probably a mixture of the two bases, C_9H_7N, and $C_{10}H_9N$ yields, on oxidation, a new acid having the formula $C_9H_9NO_3$; when treated with potash lime this acid yields aniline and ammonia only. The author of the paper thought that probably two intermediate bodies are formed, the latter of which has the same formula as indol. Prof. Dewar hopes to separate this body. This investigation shows that the bases of the pyridene series are related to the aromatic nucleus of the benzene series.

Dr. Tilden described his investigation on the *Nitrosoderivatives of the Terpenes*. So far as his experiments have gone, he has found but two different nitroso-compounds having the formula $C_{10}H_{15}NO$—one of these melts at 70°, and the other at 129°. Dr. Tilden also described a substance, isomeric with purpurin, $C_{14}H_8O_5$, produced by the action of chromic acid upon either of the aloms. Mr. Dittmar made some remarks on Reboul's paper on pyrotartaric acid; and also described at some length experiments on the analysis of coal-gas. He did not consider that the ultimate analysis of coal-gas gave any reliable information as to its illuminating power. He showed that benzene vapour may exist in coal-gas, but that by passage into an ordinary gas-holder the greater part of that vapour is removed by the water in the gas-holder. A few other papers were read relating to technical chemistry.

Altogether the section may be congratulated on having got through a fair amount of honest work.

SECTION C.—GEOLOGY.

Notices of Terraces, Flats, and Haughs at High Levels in the Carron Valley, near Falkirk, by Dr. D. Milne-Home, F.G.S.—In the region in question the author said there was highest of all, and first in point of date, a terrace of gravel 150 feet above the present sea-level. The form of this platform was due to the arranging action of water, and probably of the sea. Near its edge it is much denuded and cut into by streams, the fragments now remaining having been sometimes pared down by the action of rivers on either side into sinuous round-backed mounds which in form and structure are exactly what are known as Kaims or Eskars.

Below this level and skirting the rivers, especially the Carron and Bonny, near their confluence, are two distinct sets of haughs or alluvial flats, the one set, covered by ordinary floods and standing about ten feet above the present level of the streams, the other and older set standing 35 feet above the sea-level, and formed by the rivers, while the latter ran at a higher level than that of their present channel, a level which the author judged might be about 25 feet, allowing 10 feet for the ordinary height of floods then as now. At this period, the author maintained, the streams had not begun to cut down to their present levels, as they in all probability debouched on a sea which is now represented by the well-known "Twenty-five foot raised beach."

On the Earthquake Districts of Scotland, by Dr. James Bryce, F.G.S.—Dr. Bryce observed that there are two lines along which earthquakes are commonly observed, the one running from Inverness through the North of Ireland, to Galway Bay, and the other passing east and west through Comrie. The phenomena of earthquakes in the latter district are now being systematically observed and recorded, under the direction of a committee appointed by the British Association, seismometers being employed on the two principles of vertical pendulums and delicately poised cylinders. Arrangements have been made to ascertain whether shocks in this region can be traced to any common central point, there being reason to believe them to be connected with a mass of granite in Glen Lednoch, whose position was indicated on a map exhibited by the author.

The existence in the vicinity of Comrie of important lines of fracture in the earth's crust was pointed out, and it was suggested that these might be records of earthquakes in remote geological times. One of these lines of fracture is filled up with a dyke of basaltic rock, traceable from the Melville Monument, near Comrie, to Loch Lubnaig, and belonging to the series of dykes now regarded as of Miocene age. The other line of fracture is much older, and divides (with an enormous displacement) the Lower Old Red formation from the Metamorphic rocks of the highlands.

For the Comrie earthquakes, Dr. Bryce was inclined to accept Mr. Mallet's explanation, viz., the shock produced by the fall of masses of rock from the roof of some subterranean cavity.

As a remarkable manifestation of earthquake activity, Dr. Bryce alluded to a sudden rise of 2½ feet in the level of Loch Earn, described in a former report of the Earthquake Committee. On that occasion no change in the atmospheric pressure was indicated by the barometer. It was several hours before the motion of the lake's surface, produced by the shock, subsided.

On the Parallel Roads of Glen Roy, by Dr. D. Milne-Home, F.G.S.—Dr. Milne-Home exhibited a map showing the parallel roads as laid down by the Ordnance Survey, and the positions of the barriers necessary for the damming-up of the lake at the successive stages marked by the several beaches or "roads." The author rejected the theory of a marine origin for the beaches, and declared himself unable to accept Prof. Tyndall's view that the lakes were barred by glaciers protruding from lateral valleys.

He then went on to show that solid barriers, not of ice, but of detritus, would alone account for the phenomena in question. The cutting through of the barriers would account for the different levels of the roads. The author pointed out that in the positions where the detrital barriers must have stood, the roads stop short abruptly.

It was pointed out on the map that the detrital mounds in Glen Spean make a horse-shoe bend, with the convexity up the valley. They could not therefore have been derived from a glacier coming down Glen Spean, or from the lateral valley of Loch Treig. Mr. Milne-Home ascribed them to the droppings of icebergs floating eastward up the valley.

Mr. J. Macfadzean also read a paper *On the Parallel Roads of Glen Roy*, supporting the marine theory of their origin.

On the Geology of Foula, Shetlands, by G. A. Gibson, M.B., B.Sc.—The author had constructed from his own observations a geological map of the island, which was exhibited on the wall. A fault running north and south divides Foula into two regions of very different aspect. On the eastern or upthrow side of the fault the rock is a foliated gneiss, much folded and faulted, and copiously veined with red granite and to a less extent with grey granite. There is no granite mass *in situ* in the neighbourhood whence these veins may be supposed to have radiated. The gneiss resembles in character and also in its general strike the Laurentian of the north-west of Scotland.

On the western side of the fault the rocks are flags and sandstones identical with the Lower Old Red beds of the Shetland Islands, although in Foula no fossils have been detected in them. They dip at first at a high angle away from the fault, but gradually become flatter westwards, till they are almost horizontal at the sea. Their thickness is estimated by Mr. Gibson at 6,600 feet.

The granite dykes do not traverse the Old Red rocks.

On the Junction of Granite and Old Red Sandstone in Arran, by E. Wünsch, F.G.S.—The author described and illustrated, by diagrams, sections at Eas na Oich and Corrie, exhibiting a passage from Old Red flags and Conglomerates to the granite of the central nucleus of the island. This fact, the author said, would necessitate the alteration at the points in question, of Dr. Bryce's and Prof. Ramsay's maps, which agreed in representing the granitic nucleus as surrounded by a ring of slates, there being no slates at least as far south as Mouldon. He mentioned that everywhere at the point of contact with the Old Red Sandstone the granite was delicately mottled or clouded, as though the black film of the absorbed mass had remained floating and became fixed in the white pasty mass, and this appearance, he held, was in itself sufficient to point to a junction of granite with rock other than slate, for, though innumerable instances might be seen in other parts of the island of junctions of granite with true slate, in not a single instance was the adjoining granite affected in this particular manner.

A suite of rock specimens was exhibited showing the passage of the sedimentary rocks into granite.

On the most recent Researches into the Structure and Affinities of the Plants of the Coal Measures, by Prof. W. C. Williamson, F.R.S.—Prof. Williamson expressed his strong conviction that the flora of the Coal Measures would ultimately become the battle-field on which the question of evolution with reference to the origin of species would be fought out. There would probably never be found another unbroken period of a duration equal to that of the Coal Measures. Further, the roots, seeds, and whole reproductive structure of the Coal-measure plants are all present in an unequalled state of preservation. With reference to Calamites, Prof. Williamson said that what had formerly been regarded as such had turned out to be only casts in sand and mud of the pith of the true plant.

Brongniart believed, forty years ago, that he had established two types of the plant called calamite, one like our modern equisetums, and the other (Calamodendron) allied to the pines. Prof. Williamson, in the first of his memoirs, announced that this was an error, that there was only one generic type representing the modern equisetaceous plants, but gigantic. He had recently obtained a specimen of a calamite with the bark on, exhibiting the following structure :—

A nucleal cellular pith, surrounded by canals running lengthwise down the stem ; outside of these canals wedges of true vascular structure ; and lastly, a cellular bark.

Brongniart had further separated Lepidodendron from Sigillaria, being under the impression that a layer of exogenous growth characterises Sigillaria and is absent in Lepidodendron. But Prof. Williamson had obtained a series of young and old specimens which clearly showed that the difference is not generic, but is merely one of species, or of the age of individual plants.

Prof. Williamson also explained that the separation of the genera Asterophyllites and Sphenophyllum was uncalled for, the wedge-shaped leaf of Sphenophyllum being merely the result of the coalescence of several of the leaves of Asterophyllites.

On Labyrinthodont Remains from the Upper Carboniferous (Gas Coal) of Bohemia, by Dr. Anton Fritsch.—The gas coals of Bohemia are unusually rich in remains of Labyrinthodonts, fishes, and insects. They lie near the top of the Coal Measures, and are regarded by Dr. Fritsch as passage-beds, the fauna being of Permian and the plants of Carboniferous types.

Dr. Fritsch exhibited a series of plates, as well as his original specimens. In one Labyrinthodont the skeleton is completely ossified. A Ctenodus has the bony part of the skull preserved. A Diplodus has a perfect lower jaw, with teeth.

Among insects, one new species has the seventh pair of feet enlarged as in Pterygotus.

A new species, named by Dr. Fritsch *Ulus constans*, is interesting as showing how little the genus has changed since Palæozoic times.

On the Strata and Fossils between the Borrowdale Series and the Coniston Flags of the North of England, by Prof. Harkness, F.R.S., and Prof. A. H. Nicholson, M.D.—The authors had found an unbroken succession of the strata on this horizon at several places in the North of England, which, as exhibited in Skellgill, they tabulated as follows :—

Base of Coniston flags, with *Monograptus*, *Retiolites Geinitzii*, &c.
Knock beds, "pale slates," with casts of a small *orthis*.
Graptolitic mudstones with a grey band full of brachiopods, &c.
Coniston limestone and shale—the shale highly fossiliferous.
Traps, the summit of Borrowdale Group, with ash beds containing rust cavities ("Stylend grassing beds").

These deposits must be for the most part Lower Silurian. Below them are the Skiddaw slates, containing well-marked graptolites. The Skiddaw slates are found neither in Scotland nor Ireland.

The Tarranon shales, which are 300 feet thick in South Wales, develop in the North to a thickness of 1,500 feet, and the Geological Survey has mapped them as conformable to the Bala beds.

South of Bala Lake, Lower Llandovery rocks get in between the Tarranon shales and the underlying Bala beds. Still further to the south the Upper Llandovery comes in.

The authors conclude, therefore, that the Tarranon shales of the North represent also the Upper and Lower Llandovery rocks. They consider also that the Lower Llandeilo of the Southern Uplands of Scotland, estimated by the Geological Survey to have a thickne s of 20,000 feet, is represented in the North of England by contemporaneous igneous rocks.

Notes on the Drifts and Boulders of the Upper Part of the Valley of the Wharfe, Yorkshire, by the Rev. E. Sewell, M.A., F.G.S.—In this region there are two boulder clays, the lower blue and hard, with many glaciated stones, and the upper, and more generally diffused, yellow and looser, and with comparatively few glaciated stones. In the blue clay there are many boulders from the north-west, while those of the yellow clay are for the most part of the local Millstone Grit.

In the upper part of the valley the clays are largely concealed by gravel and sand, which attain a thickness of 150 feet. This deposit appears to graduate into, and alternate with, the underlying yellow boulder-clay. It rises here and there into crooked eskar-mounds. It contains pebbles and boulders mostly of the local Millstone Grit, but there are also some of Carboniferous Limestone.

The Valley of the Wharfe must have been filled up with gravelly drift to a certain height, and then (in post-glacial times) must have commenced the excavation of the present valley.

The author thinks that the theory of a marine origin for the gravel best accounts for the phenomena it presents. The boulders may have been dropped from floating ice.

Above the valley, on the hills of Millstone Grit, there occur boulders of limestone which must have come from the north-west, crossing intervening valleys and ridges. The boulders reach the height of 1,200 feet. There are no erratics on the eastern side of the Pennine Hills above 1,250 feet, but on the western slope they occur at greater heights.

On Ridgy Structure in Coal, with Suggestions towards accounting for its Origin, by Prof. James Thomson, F.R.S.E.—The coal in question was exhibited by the author, and was derived from South Wales. It presented the appearance in miniature of a number of sharp, serrated, labyrinthine mountain ridges. Prof. Thomson suggested that the coal-seam might have diminished in weight owing to the escape of fire-damp, and that thereupon the pressure of the overlying strata might have reduced its bulk, a double series of oblique fissures allowing the upper half of the seam to interlock with the lower half. Experiments on the behaviour of cast-iron columns under pressure had demonstrated the possibility of such fissures.

Further Illustration of the Jointed Prismatic Structure in Basalts and other Igneous Rocks, by Prof. James Thomson, F.R.S.E.—Prof. Thomson suggested that the structure in question might have been induced by the accidental presence of foreign substances in the molten rock. The paper was illustrated by specimens of ochreous clay, and of bricks and fire-clay used in melting gold in the Royal Mint.

SECTION D.—BIOLOGY.

After the delivery of the President's Address, Dr. Hooker, in proposing a vote of thanks to him, said that the President should not have termed his address an excursion into the bye-paths of biology, but rather a discovery and exposition of the true value of many small facts hitherto considered trivial. Mr. Darwin and Mr. Wallace were the men who were utilising the "waste observations of biology." He entirely agreed with Mr. Wallace as to the great importance of animal life to the colouration of flowers, but perhaps a broader aspect still was to be thought of in that connection—the influence of climate, the chemical rays of the sun, and cloudy weather. Thus brightly-coloured flowers were much more numerous in the eastern than in the western districts of Great Britain. Again, the further islands were from great continents, the less conspicuous colouration was possessed by their flowers, as a rule.

Department of Anthropology.

Several papers were read bearing upon the Highland race and language. Mr. Hector McLean was of opinion that there was not sufficient basis for the view that the primitive continental Celts were divided into two branches, Gaelic and Cymric. It was perhaps more reasonable to consider the ancient Celtic language as possessing several dialects, varying gradually from the Baltic to the Mediterranean and from the Alps to the West of Ireland. Mr. McLean thought there was a tendency to consider the Celtic languages more Aryan than they really were, and he gave a list of words from non-Aryan languages having a close resemblance in form to Celtic words. The Gaelic language now fringed the whole west of the British Isles, with considerable though gradual dialectical differences. South Kintyre was nearer in language to Antrim than to Skye. He believed that Kerry men and Sutherlanders would not require long intercourse in order to be able to understand each other. Mr. McLean also noticed a number of the physical characteristics of the Western Highlanders, from which he inferred that they had been materially influenced as a race by the Norwegian occupation from the eighth to the thirteenth century. He had looked at Danish, Swedish, and Norwegian sailors side by side with Western Highlanders, and had been surprised at the resemblances between the former and the fair individuals of the latter. Local names of Norse origin were found in all the isles and all along the coast line. His general conclusions were that the Highlanders of the present day were derived from a commixture of several races, pre-Celtic, Celtic, and Scandinavian, and it

would seem that there must have been three or four pre-Celtic stocks. Another paper by Mr. McLean was *On the Anglicising and Gaelicising of Surnames.*

Dr. Phené read a paper *On Recent Remains of Totemism in Scotland.* He defined Totemism as a form of idolatry; a totem was either a living creature or a representation of one, mostly an animal, very seldom a man. It was considered, from reference to Pictish and other devices, that a dragon was a favourite representative among such people of Britain as had not been brought under Roman sway.

Mr. W. J. Knowles, of Belfast, gave a further account of the prehistoric discoveries made at Port Stewart, near Londonderry. They were found in pits excavated by the wind among the sand-hills. The remains included arrow-heads, scrapers, hammers, flakes, bone implements, and bones of the horse, ox, pig, dog, &c., together with edible shells, all mixed up together, and apparently of the same age. As late as the 20th of July last the author and two companions had found, in less than four hours, three arrow-heads, two beads, thirty or forty scrapers, and several hammer-stones, as well as bones which bore marks of cutting or sawing. One of the most interesting of recent finds was about a dozen very small stone beads, found within a few yards' radius. They were concave on one side and convex on the other. Mr. Knowles had tested the cutting power of the flint implements on a common beef-bone, using a little water, and he found that he cut through into the hollow of the bone in fourteen minutes; he had also bored a hole through a bone with a piece of flint. The marks made by the flints on recent bones were very similar to those found on the ancient bones. Mr. Knowles also read a paper *On the Classification of Arrow-heads*, recommending the use of the following terms:—Stemmed, indented, triangular, leaf-shaped, kite-shaped, and lozenge-shaped. Commander Cameron mentioned that arrow-heads of the same shape as many exhibited by Mr. Knowles were in use in various African tribes. One shape was formed so as to cause the arrow to rotate, and was principally used for shooting game at long distances. The shape of the arrows varied according to the taste of the makers; in one district there were forty or fifty different shapes.

Commander Cameron read part of a paper by Capt. J. S. Hay, relative to a strange malformation among people in the district of Akem, West Africa, the first announcement of which was received with some incredulity. The malformation in question is confined to the male sex, and consists in a protuberance or enlargement of the cheek bones under the eyes, taking the form of horns on each side of the nose. The malformation begins in childhood, but does not appear to be hereditary. It presents no resemblance to a diseased structure, nor is it a raised cicatrix. An endeavour is being made to procure skulls in which the phenomenon appears, for exhibition to scientific men in London.

Miss A. W. Buckland's paper *On Primitive Agriculture*, was very highly commended by Col. Lane Fox. We can only state her general conclusion that cereals were introduced by pre-Aryan races of common descent over a very wide range of the world; and they also introduced the worship of the moon as an agricultural deity. The absence of agricultural implements in prehistoric remains proved their extreme simplicity; probably only a pointed stick was used, a form still persistent. Some of the stone celts may have been used as hoes, and flint flakes might also have been inserted in wooden frames for use as harrows. Furrows and ridges seemed to have been everywhere used.

Department of Anatomy and Physiology.

A valuable series of researches on certain special poisons was presented from the Owens College Laboratory, in papers separate or conjoint, by Prof. Gamgee, F.R.S., Mr. Leopold Larmuth, and Dr. John Priestley. Vanadium and its compounds have been specially investigated, and found to be irritant poisons, rapidly causing death, often preceded by paralysis, convulsions, &c. When much diluted the solutions act injuriously on bacteria, germinating seeds, fungi, &c. The results are the same whether the solution is injected into the skin, the veins, or the alimentary canal of higher animals. Both before and after division of the respiratory nervous centre, vanadium causes in the first instance a stimulation, and in the next a depression of respiration. When the muscles and nerves of a frog poisoned with vanadium were tested by electricity after reflex irritability was entirely destroyed, the work done by the muscles showed no differences from that of non-poisoned muscles. The action of vanadium on the heart of frogs is curious; when vanadium is injected, the inhibitory centres acting on the auricles are not affected, but the vagus nerve loses its power of inhibiting the contraction of the ventricle. This result causes a dilemma which cannot yet be resolved, for it appears that vanadium is not a poison of the muscular fibres. Experiments have also been made on the relative poisonous activities of the ortho-, meta-, and pyro-phosphoric acids and their compounds, and they have been found to vary considerably in their intensity. Further, a relationship in the various phenomena produced has been made out between the different phosphates and vanadates. Investigations relating to chromium, in which rabbits, guinea-pigs, and frogs were employed, demonstrate considerable differences in its physiological action from that of vanadium. At first it induces irritation of the alimentary mucous membrane, and secondly it acts directly on the principal nervous centres, causing convulsions, paralysis, vomiting, a fall of blood pressure, and a sudden and temporary stoppage of the heart in dilatation. It is not specially a poison of muscle or of nerve-trunks.

In the discussion which followed the reading of these papers, Prof. Kronecker, of Leipzig, expressed his opinion that the vanadates were really poisons of the muscular substance of the heart, and he accounted for the differences between the action on the auricle and ventricle by supposing a certain difference between the muscular substance of these two chambers. Dr. McKendrick, who presided in this department, said that Prof. Gamgee's researches showed the advantage of the combination of the highest chemical with physiological knowledge, and they led to the hope that ultimately some definite laws would be discovered regulating the relations between chemical constitution and physiological action. The field of inorganic chemistry was a very fertile one for this purpose, and much more likely to yield great results of this kind than the more complex considerations of organic chemistry. One important result was confirmed by Prof. Gamgee's investigations, that the larger the molecule of a substance the more powerful was its operation, but this was affected also by the stability of the molecule.

Prof. Gamgee also read a paper *On the Changes of Circulation which are observed when Blood is expelled from the Limbs by Esmarch's Method.* The experiments were conducted on healthy students. When the blood was expelled from one leg the heart beat more rapidly, but only for a short time, and the same result followed the application of the bandage to the second leg. When the heart began to beat at its usual rate the tourniquets were loosened, and in an instant the limbs, previously blanched, became suffused with a blush, while sensibility therein became more and more blunted, and the heart bounded off at an exceedingly rapid rate, to return, however, to its normal beat almost immediately. It has been suggested that the increase of the heart's beat when the bandage is applied is intimately connected with the diminution of the normal difference between arterial and venous pressure. It appears likely that an increase of pressure on the right side of the heart tends to quickening the beating of the heart, and the increase of rapidity on removing the bandage round the limb was no doubt the result of the sudden diminution of arterial pressure thus caused. Prof. Kronecker desired that it should not be lost sight of that the altered chemical composition of the blood also had some influence in this matter.

Dr. Stirling, of Edinburgh, gave a very lucid account of his discovery of small nerve ganglia in many parts of the lung, and especially in relation to the bronchi at the base of the lung. These small collections of ganglion cells may be either in the course of the nerves or at their forks. They are directly continued by two extremities into the gray or sympathetic nerve-fibres. Dr. Stirling believed that these were local nerve-centres for the muscular fibres of the blood-vessels, controlling their calibre, and thus regulating the amount of blood passing through them. Dr. Gardner threw out the idea that these local nerve-centres might have another function, that of regulating the capacity of different bronchi, and so varying the amount of air admitted to or expelled from particular regions of the lung. He had long believed that some such arrangement must exist, in consequence of stethoscopic observations both on the healthy and the diseased subject. Dr. Stirling suggested that this regulating power might reside in the higher nervous centres, for stimuli could be sent down through any limited number of fibres of the whole respiratory nerves. Many of the distinguished physiologists present expressed high praise of Dr. Stirling's abilities as shown in this research.

Department of Zoology and Botany.

Mr. J. Gwyn Jeffreys, F.R.S., gave an account of the biological results of the voyage of the *Valorous* to Disco Island in 1875, which will be published in full in the *Proceedings* of the Royal Society. He urged the importance of repeated expeditions of this kind. A century of hard work would not suffice to collect all the information that was needed. Hitherto naturalists had only scraped the bottom of a few acres out of the many millions of square miles of the ocean. The British nation had hitherto done very little for submarine discovery in proportion to the poorer countries of Scandinavia, which had sent out expedition after expedition, yielding the most valuable results to science. Unfortunately, the latest intelligence as to the present Norwegian enterprise was that their work had been much interfered with by tempestuous weather. An important result of Mr. Jeffreys' experience was the bringing up of large and small stones, some very sharp, from the sea-bottom, at great depths. He thought telegraphic engineers had not taken this sufficiently into account in the construction of cables, having proceeded as if they had only to deal with an entirely soft bottom. The number of species of mollusca obtained by the *Valorous* was 183, of which forty were new to science. His opinion, derived from personal knowledge of the American, as well as of the European, fauna, was that the submarine fauna of Davis' Straits was predominantly European, although a number of American forms were found with them. An interesting feature was the discovery of a number of species previously only known in a fossil state in Tertiary rocks far distant, as in the Mediterranean; other species were remarkable because it was now for the first time shown what an enormous range in space and latitude they had, sometimes at least 1,200 miles. Dr. McIntosh, of St. Andrews, Prof. Dickie, of Aberdeen, and Dr. Carpenter gave addresses respectively on the Annelids, the Diatoms, and the Arenaceous Foraminifera brought home by the *Valorous*, and confirmed Mr. Gwyn Jeffreys in maintaining the predominance of European forms.

Mr. John Murray gave an address on oceanic deposits and their origin, based on observations on board the *Challenger*. He described and exhibited specimens of various kinds of deep-sea deposits. He did not think the detritus of the modern land was carried more than two or three hundred miles from the shore. A novel constituent of the deepest sea-bottoms was pumice dust, which had been found in almost every region, arising from submarine volcanic action. Mr. Murray thought he had never failed to find a piece of pumice, when it was carefully looked for in any of the dredgings, and he believed it to be the chief origin of the deep-sea clays. Another element which appeared to have been detected at great depths was "cosmic dust," or dust formed from aërolites. Another interesting point was that whenever they got into deep water, they found manganese peroxide in nodules inclosing organic remains—sharks' teeth and pieces of bone. This formation seemed to be connected with the disintegration of volcanic rocks. Mr. Murray also discussed the question whether true equivalents of the deep-sea deposits now made known were to be found in the series of stratified rocks. If this were not the case, then it must be held that the great continents had remained substantially the same throughout a vast length of time.

FORCE [1]

At short notice it was not to be expected that I could produce a lecture which should commend itself to the Association by its novelty or originality. But in science there are things of greater value than even these—namely definiteness and accuracy. In fact without them there could not be any science except the very peculiar smattering which is usually (but I hope erroneously) called "popular." It is vain to expect that more than the elements of science can ever be made in the true sense of the word popular; but it is the people's *right* to demand of their teachers that the information given them shall be at least definite and accurate, so far as it goes. And as I think that a teacher of science cannot do a greater *wrong* to his audience than to mystify or confuse them about fundamental principles, so I conceive that wherever there appears to to be such confusion it is the *duty* of a scientific man to endeavour by all means in his power to remove it. Recent criticisms of works in which I have had at least a share, have shown me that, even among the particularly well-educated class who write for the higher literary and scientific journals, there is wide-spread ignorance as to some of the most important elementary principles of physics. I have therefore chosen, as the subject of my lecture to-night, a very elementary but much abused and misunderstood term, which meets us at every turn in our study of natural philosophy.

I may at once admit that I have nothing 'new' to tell you, nothing which (had you all been properly taught, whether by books or by lectures) would not have been familiar to all of you. But if one has a right to judge of the general standard of popular scientific knowledge from the statements made in the average newspaper—or even from those made in some of the most pretentious among so-called scientific lectures—there can be but few people in this country who have an accurate knowledge of the proper scientific meaning of the little word Force.

We read constantly of the so-called "Physical Forces"—heat, light, electricity, &c.—of the "Correlation of the Physical Forces," of the "Persistence or Conservation of Force." To an accurate man of science all this is simply error and confusion, and I have full confidence that the inherent vitality of truth will render the attempt to force such confusion upon the non-scientific public quite as futile as the hopelessly ludicrous endeavour of the *Times* to make us spell the word chemistry with a Y instead of an E. It is true that in matters such as this last a good deal depends (as Sam Weller said) "on the taste and fancy of the speller"—and sometimes even absolute error is of little or no consequence. But it is quite another thing when we deal with the fundamental terms of a science. He who has not exactly caught their meaning, is pretty certain to pass from chronic mistakes to frequent blunders, and cannot possibly acquire a definite knowledge of the subject.

In popular language there is no particular objection to multiple meanings for the same word. The context usually shows exactly which of these is intended—and their existence is one of the most fertile sources of really good puns, such as those of Hood, Hook, or Barham. And there is no reason to object to such phrases as the *force of habit*, the *force of example*, the *force of circumstances*, or the *force of public opinion*. But when we read, as I did last week, in one newspaper, that the "force" of a projectile from the 81-ton gun has at last reached the extraordinary amount of 1,450 feet, in another that the "force" of a ball from the great Armstrong gun, lately made for the Italian government, is expected to average somewhere about 30,000 foot-tons—and in a third that the water in the boiler of the *Thunderer* "would in a second of time generate a 'force' sufficient to raise 2,000 tons one foot high"—we see that there must be, somewhere at least, if not everywhere, a most reckless abuse of language. In fact we have come to what ought to be scientific statements, and *there* even the slightest degree of unnecessary vagueness is altogether intolerable.

Perhaps no scientific English word has been so much abused as the word "force." We hear of "Accelerating Force," "Moving Force," "Centrifugal Force," "Living Force," "Projectile Force," "Centripetal Force," and what not. Yet, as William Hopkins, the greatest of Cambridge teachers, used to tell us—"Force is Force"—*i.e.*, there is but one idea denoted by the word, and all force is of one kind, whether it be due to gravity, magnetism, or electricity. This alone serves to give a preliminary hint that (as I shall presently endeavour to make clear to you) there is probably no such *thing* as force at all! That it is, in fact, merely a convenient expression for a certain "rate." If anyone should imagine that "3 per cent." is a sum of money, he will soon be grievously undeceived. "3 per cent." means nothing more nor less than the vulgar fraction $\frac{3}{100}$. True, the "*Three Per Cents*" usually means something very substantial—but there the term is not a scientific one. Think for a moment how utterly any one of you, supposed altogether ignorant of shipping, would be puzzled by such a newspaper heading as "*The White Star-Line*" or "*The Red Jacket-Clipper.*" No doubt some of our scientific terms approach as near to slang as do these; but we are doing our best to get rid of them.

A good deal of the confusion about Force is due to Leibnitz and some of his associates and followers, who, whatever they may have been as mathematicians, were certainly grossly ignorant of some elementary parts of dynamics, insomuch that Leibnitz himself is known to have considered the fundamental system of the *Principia* to be erroneous, and to have devised another and different system of his own. This fact is carefully kept back now-a-days, but it *is* a fact, and (as I have just said) has had a great deal to do with the vagueness of the terms for *Force* and *Energy* in some modern languages. In fact, in their modern

[1] Evening lecture by Prof. Tait at the Glasgow meeting of the British Association, Sept. 8.

dress, the *Vis Viva*, *Vis Mortua*, and *Vis Acceleratrix* of that time have, in some of their Protean shapes, hooked themselves like Entozoa into the great majority of our text-books.

Before dealing more definitely with the proper meaning of the word "Force" I must briefly consider how we become acquainted with the physical world, and how consequently it is more than probable that some of our most profound impressions, if uninformed, are completely erroneous and misleading.

In dealing with physical science it is absolutely necessary to keep well in view the all-important principle that—

Nothing can be learned as to the physical world save by observation and experiment, or by mathematical deductions from data so obtained.

On such a text, volumes might be written; but they are unnecessary, for the student of physical science feels at each successive stage of his progress more and more profound conviction of its truth. He must receive it, at starting, as the unanimous conclusion of all who have in a legitimate manner made true physical science the subject of their study; and, as he gradually gains knowledge by this—*the only*—method, he will see more and more clearly the absolute impotence of all so-called metaphysics, or *à priori* reasoning, to help him to a single step in advance.

Man has been left entirely to himself as regards the acquirement of physical knowledge. But he has been gifted with various *senses* (without which he could not even know that the physical world exists) and with *reason* to enable him to control and understand their indications.

Reason, unaided by the senses, is totally helpless in such matters. The indications given by the senses, unless interpreted by reason, are utterly unmeaning. But when reason and the senses work harmoniously together, they open to us an absolutely illimitable prospect of mysteries to be explored. This is the test of true science—there is no resting-place—each real advance discloses so much that is new and easily accessible that the investigator has but scant time to co-ordinate and consolidate his knowledge before he has additional materials poured into his store.

To sight without reason, the universe appears to be filled with light—except, of course, in places surrounded by opaque bodies.

Reason, controlling the indications of sense, shows us that the sensation of light is our own property; and that what we understand by brightness, &c., does not exist outside our minds. It shows us also that the sensation of colour is purely subjective, the only difference possible between different so-called rays of light outside the eye being merely in the extent, form, and rapidity of the vibrations of the luminiferous medium.

To hearing, without reason, the air of a busy town seems to be filled with sounds. Reason, interpreting the indications of sense, tells us that if we could SEE the particles of air, we should observe among them simply a comparatively slow agitation of the nature of alternate compressions and dilatations superposed upon their rapid motions among one another. And our classification of sounds as to loudness, pitch, and quality, is merely the subjective correlative of what in the air-particles is objectively the amounts of compression, the rapidity of its alternations, and the greater or less complexity of the alternating motion.

A blow from a stick or a stone produces pain and a bruise; but the motion of the stick or stone before it reached the body is as different from the sensation produced by the blow as is the alternate compression and dilatation of the air from the sensation of sound, or the etherial wave-motion from the sensation of light.

Hence to speak, as the great majority even of "educated" people do, of what we ordinarily mean by light or sound, as existing outside ourselves, is as absurd as to speak of a swiftly-moving stick or stone as pain. But no inconvenience is occasioned if we announce the intention to use the terms light and sound for the objective phenomena, and to speak of their subjective effects as "luminous impressions" or "noise," as the case may be. In this case there is outside us energy of motion of every kind, but in the mind mere corresponding impressions of brightness and colour, noise or harmony, pain, &c., &c.

As another instance, it is obvious that we must be extremely cautious in our interpretation of the immediate evidence of our own senses as to heat.

Touch, in succession, various objects on the table. A paper-weight, especially if it be metallic, is usually cold to the touch; books, paper, and especially a woollen table-cover, comparatively warm. Test them, however, by means of a *thermometer*, not by the sense of touch, and in all probability you will find little or no difference in what we call their *temperatures*. In fact, any number of bodies of any kind shut up in an inclosure (within which there is no fire or other source of heat) all tend to acquire ultimately the same temperature. Why, then, do some feel cold, others warm to the touch?

The reason is simply this—the sense of touch does *not* inform us directly of temperature, but of *the rate at which our finger gains or loses heat*. As a rule bodies in a room are colder than the hand, and heat always tends to pass from a warmer to a colder body. Of a number of bodies, all equally colder than the hand, that one will seem coldest to the touch which is able *most rapidly* to convey away heat from the hand. The question, therefore, is one of *conduction of heat*. And to assure ourselves that it is so, reverse the process: *let us*, in fact, *try an experiment*, though an exceedingly simple one; for the essence of experiment is to modify the circumstances of a physical phenomenon so as to increase its value as a test. Put the paper-weight, the books, and the woollen table-cloth into an oven, and raise them all to one and the same temperature—considerably above that of the hand. The woollen cloth will still be comparatively cool to the touch, while the metal paper-weight may be much too hot to hold. The order of these bodies, as to warm and cold, in the popular sense, is in fact reversed; and this is so because the hand is now *receiving* heat from all the various bodies experimented on, and it receives most rapidly from those bodies which in their previous condition were capable of abstracting heat most rapidly. However it may be in the moral world, in the physical universe the giving and taking powers of one and the same body are strictly correlative and equal.

Thus the direct indications of sense are in general utterly misleading as to the relative temperatures of different bodies.

In a baker's oven, at temperatures far above the boiling point of water (on one occasion even 320° F., so high indeed that a beef-steak was cooked in thirteen minutes), Tillet in France, and Blagden and Chantrey in England, remained for nearly an hour in comparative comfort. But though their clothes gave them no great inconvenience, they could not hold a metallic pencil-case without being severely burned.

On the other hand, great care has to be taken to cover with hemp, or wool, or other badly conducting substance, every piece of metal which has to be handled in the intense cold to which an Arctic expedition is subjected; for contact with very cold metal produces sores almost undistinguishable from burns, though due to a directly opposite cause. Both of these phenomena, however, ultimately depend on the comparative facility with which heat is conducted by metals.

Even from the instance just given, you cannot fail to see that there is a profound distinction between heat and temperature. Heat, whatever it may be, is SOMETHING which can be transferred from one portion of matter to another; the consideration of temperatures is virtually that of the mere CONDITIONS which determine whether or not there shall be a transfer of heat, and in which direction the transfer is to take place. Bear this carefully in mind, because it has most important analogies to the results we meet with in considering the nature of Force.

It has been definitely established by modern science that *heat, though not material, has objective existence in as complete a sense as matter has.*

This may appear, at first sight, paradoxical; but we must remember that so-called paradoxes are merely facts as yet unexplained, and therefore still apparently inconsistent with others already understood in their full significance.

When we say that matter has objective existence, we mean that it is something which exists altogether independently of the senses and brain-processes by which alone we are informed of its presence. An exact or adequate conception of it, if it could be formed, would probably be something very different from any conception which our senses will ever enable us to form; but the object of all pure physical science is to endeavour to grasp more and more perfectly the nature and laws of the external world, using the imperfect means which are at our command—reason acting as interpreter as well as judge, while the senses are merely more or less untrustworthy and incompetent witnesses, but still of inconceivable value to us because they are our only available ones.

Without further discussion we may state once for all that our conviction of the objective reality of matter is based mainly upon the fact, *discovered solely by experiment*, that we cannot in the slightest degree alter its quantity. We cannot destroy, nor can we produce, even the smallest portion of matter. But reason

requires us to be consistent in our logic; and thus, if we find anything else in the physical world whose quantity we cannot alter, we are bound to admit it to have objective reality as truly as matter has, however strongly our senses may predispose us against the concession. Heat therefore, as well as light, sound, electricity, &c., though not forms of matter, must be looked upon as being as real as matter, simply because they have been found to be forms of energy—which in all its constant mutations satisfies the test which we adopt as conclusive of the reality of matter. We shall find that this test fails when applied to force.

But you must again be most carefully warned to distinguish between heat and the mere sensation of warmth; just as you distinguish between the motion of a cudgel and the pain produced by the blow. The one is the *thing* to be measured, the other is only the more or less imperfect reading or indication given by the instrument with which we attempt to measure it in terms of some one of its effects. So that when your muscular sense impresses on you the notion that you are exerting force as in pushing or pulling, you ought to be very cautious in forming a judgment as to what is really going on; and you ought to demand much farther evidence before admitting the objective reality of force.

Until all physical science is reduced to the deduction of the innumerable mathematical consequences of a few known and simple laws, it will be impossible altogether to avoid some confusion and repetition, whatever be the arrangement of its various parts which we adopt in bringing them before a beginner. But when we confine ourselves to one definite branch of the subject, all of whose fundamental laws can be distinctly formulated, there need be no such confusion. Here in fact the mathematician has it all in his own hands. He is the skilled artificer with his plan and his trowel, and the hodmen have handed up to him all the requisite bricks and mortar.

[Prof. Tait then gives a quotation in support of this view.]

Whether there is such a *thing* as force or not I shall consider presently. But in the meanwhile there can be no doubt that it is a convenient term, provided it be employed in one definite sense, and one only. Let us then first see how it is to be correctly used. Here we cannot but consult Newton. The sense in which he uses the word "force," and therefore the sense in which we must continue to use it if we desire to avoid intellectual confusion, will appear clearly from a brief consideration of his simple statement of the laws of motion.

The first of these laws is: *Every body continues in its state of rest or of uniform motion in a straight line, except in so far as it is compelled by impressed forces to change that state.*

In other words, any change, whether in the *direction* or in the *rate* of motion of a body is attributed to *force*. Thus a stone let fall moves quicker and quicker, and we say that a force (viz., the weight of the stone, or the earth's attraction for it) is continually acting so as to increase the *rate* of the motion. If the stone be thrown upwards, the *rate* of its motion continually diminishes, and we say that the same force (the stone's weight) is continually acting so as to produce this diminution of speed. So far, none of you probably feels the least difficulty. But we have got only half of the information on this point which Newton's first law affords. You see the moon revolving about the earth, and the earth and other planets revolving about the sun—approximately, at least, in circles. Why is this? Their *directions* of motion are constantly changing; in fact, a curved line is merely a line whose direction changes from point to point, while a straight line is one whose direction does not change; but to produce this change of direction force is required just as much as to produce change of speed. That is supplied by the gravitation attraction of the central body of the system. The old notion was that a centripetal force was required to balance the so-called centrifugal force, it being imagined that a body moving in a circle had a tendency to fly outwards from the centre! Newton's simple law exposes fully the absurdity of this. If a body is to be made to move in a curved line instead of its natural straight path, you must apply force to compel it to do so; certainly not to prevent it from flying outwards from the centre, about which it is for the moment revolving. In fact, inertia means, not revolutionary activity, but dogged perseverance, and just as you must apply force *in* the direction of motion to change the *rate* of motion, so must you apply force *perpendicular* to the direction of motion to change that *direction*.

Newton's second law is now required: *Change of motion is proportional to the impressed force, and takes place in the direction of the straight line in which the force acts.*

Mark here most carefully that this one simple law holds for *all* kinds of force alike. There is no special law for gravitation-force and others for electric and magnetic forces. All are defined alike, without reference to their origin.

Motion, as Newton has previously defined it, is here used as a technical scientific term for what we now call *momentum*. It is the product of the mass moving into the velocity with which it moves. "Change of motion," therefore, is change of momentum, or the product of the mass of the moving body into its change of velocity. Now a change of velocity is itself a velocity, as we see by the science of mere motion—kinematics—the purely mathematical science of mixed space and time.

Newton's words, however, imply more than this. Of course, the longer a given force acts, the greater will be the change of momentum which it produces; so that to compare forces, which is the essence of the process of measuring them, we must give them equal times to act—or, in scientific language, we must measure a force by the *rate* at which it produces change of momentum. Rate of change of velocity is called in kinematics acceleration. Thus the measure of a force is the product of the mass of the body moved into the acceleration which the force produces in it. This is the so-called *Vis motrix*, or "moving force" of the Cambridge text-books:—the so-called *Vis acceleratrix*, or "accelerating force," being really no force at all, but another name for the kinematical quantity acceleration which I have just defined.

Unit force is thus that force which, *whatever be its source*, produces unit momentum in unit of time. If we employ British units—unit of force is that which, in one second, gives to one pound of matter a velocity of one foot per second. Here you must carefully notice that a *pound* of matter is a certain *mass* or quantity of matter. When you buy a pound of tea, you buy a quantity of the matter called tea, equal in *mass* to the standard pound of platinum. The idea of weight does not enter primarily into the process. In fact, the use of an ordinary balance depends upon one clause of Newton's law of gravitation—which tells us that in any locality whatever, the weights of bodies are equal if their masses are equal. The weight of a pound of matter varies from place to place on the earth's surface—it depends on the attracting as well as the attracted body. The mass of a body is its own property. The earth's attraction for a body, or the weight of the body, is a force which produces in it in one second, a velocity which (in this latitude, and at the sea-level) is about 32·2 feet per second. So that, in Glasgow the weight of a pound —which we take as our standard of mass—is rather more than thirty-two units of force, or, what comes to the same thing, the British unit of force is about the former weight of a penny letter —half an ounce.

Some people are in the habit of confounding force with momentum. No one having sound ideas of even elementary mathematics could be guilty of this or any similar monstrosity. He would as soon, as Hopkins used to say, measure heights in acres, or arable land in cubic miles. But to show to a non-mathematician that it is really monstrous to confound force and momentum, it suffices to change the system of units employed in measuring them, when it will be found that, if numerically equal for any one system of units, they are necessarily rendered unequal by a mere change of the unit employed for time. Now two things which are really equal to one another must necessarily be expressed by the same numerical quantity *whatever* system of units be adopted. Let us try then unit of force and unit of momentum, as defined by pound, foot, second, units: and see what alterations a common change of these fundamental units will make in their numerical expression.

Unit momentum is that of one pound of matter moving with a velocity of one foot per second. Unit force is that force which, acting for one second, produces in unit of mass a velocity of one foot per second. In each of these statements you may put an ounce or a ton, instead of a pound, and an inch or a mile in place of a foot, and their relative value will not be altered. But suppose we take a minute instead of a second as the unit of time. One foot per second is sixty feet per minute—so this change of the time unit increases sixty-fold the nominal value of the momentum considered. But in the case of the force our statement would stand thus:—What we formerly called unit of force is that which, acting for one-sixtieth only of our new unit of time produces in a mass of one pound, sixty-fold the new unit of velocity. In other words the number expressing the momentum is increased sixty-fold, while that representing the force is increased three thousand six hundred fold.

In fact, whatever be the system of units you employ—if you increase in any proportion the unit of time, the measure of a

momentum is increased, in that proportion simply, while that of a force is increased in the duplicate ratio. The two things are, therefore, of quite dissimilar nature, and cannot lawfully be equated to one another under any circumstances whatever.

The mathematician expresses this distinction at once by saying that momentum is the time-integral of force, because force is the rate of change of momentum.

But what I have already said as to the meaning of Newton's two first laws leaves absolutely no doubt as to the only definite and correct meaning of the word force. It is obviously to be applied to any pull, push, pressure, tension, attraction, or repulsion, &c., whether applied by a stick or a string, a chain or a girder; or by means of an invisible medium such as that whose existence is made certain by the phenomena of light and radiant heat, and which has been shown with great probability to be capable of explaining the phenomena of electricity and magnetism.

I have already mentioned to you that the notion of force is suggested to us by the so-called muscular sense, which gives us a peculiar feeling of pressure when we attempt to move a piece of matter. To get a notion of what it really means we must again have recourse to physical facts instead of the uncontrolled evidence of the senses. Almost all that is required for this purpose is summed up for us in the remaining law of motion. Before we take it up, however, let us briefly consider the position at which we have arrived.

We have seen how to get rid of two gratuitous absurdities—the so-called centrifugal force and accelerating force, and we must proceed to exterminate living force. Cormoran and Blunderbore have been disposed of, but a more dangerous giant remains. More dangerous because he is a reality, not a phantom like the other two. Whatever force may be, there is no such thing as centrifugal force; and accelerating force is not a physical idea at all. But that which is denoted by the term living force, though it has absolutely no right to be called force, is something as real as matter itself. To understand its nature we must have recourse to another quotation from the *Principia*.

Newton's third law of motion is to the effect that—

"*To every action there is always an equal and contrary reaction; or, the mutual actions of any two bodies are always equal and oppositely directed.*"

This law Newton first shows to hold for ordinary pressures, tensions, attractions, impacts, &c., that is for *forces* exerted on one another by two bodies, or their time-integrals. And when he says—"If any one presses a stone with his finger his finger is pressed with an equal and opposite force by the stone," we begin to suspect that force is a mere name—a convenient abstraction—not an objective reality.

Pull one end of a long rope, the other being fixed. You can produce a practically *infinite* amount of force, for there is stress across every section throughout the whole length of the rope. Press upon a movable piston in the side of a vessel full of fluid. You produce a practically infinite amount of force—for across every ideal section of the liquid a pressure per square inch is produced equal to that which you applied to the piston. Let go the rope, or cease to press on the piston, and all this practically infinite amount of force is gone!

The only man who, to my knowledge, ever tried to discover experimentally what might be correctly called *conservation of force*, was Faraday. He was not satisfied with the mode of statement of Newton's law of gravitation, in which the mutual attraction between two bodies is said to VARY inversely as the square of their distance from one another. When the distance between two bodies is doubled, their mutual attraction falls off to one-fourth of what it formerly was. Faraday seriously set to work to determine what became of the three-fourths which have disappeared, but all his skill was insufficient to give him any result. Faraday's insight was so profound that we cannot assert that something may not yet be discovered by such experiments, but it will assuredly not be a conservation of force.

But Newton proceeds to point out that this third law is true in another and much higher sense. He says:—

"*If the action of an agent be measured by the product of its force into its velocity; and if, similarly, the reaction of the resistance be measured by the velocities of its several parts into their forces, whether these arise from friction, cohesion, weight, or acceleration, action and reaction, in all combinations of machines, will be equal and opposite.*"

The actions and reactions which are here stated to be equal and opposite, are no longer simple forces, but the *products* of forces into their velocities; *i.e.*, they are what are now called *rates of doing work*; the time-rate of increase, or the increase per second of a very tangible and real SOMETHING, for the measurement of which rate Watt introduced the practical unit of a *horse-power*, or the rate at which an agent works when it lifts 33,000 pounds 1 foot high per minute against the earth's attraction.

Now think of the difference between raising a hundredweight and endeavouring to raise a ton. With a moderate exertion you can raise the hundredweight a few feet, *and in its descent it might be employed to drive machinery, or to do some other species of work.* But tug as you please at the ton, you will not be able to lift it; and therefore, after all your exertion, it will not be capable of doing any work by descending again.

Thus it appears that *force* is a mere name, and that the *product of a force into the displacement of its point of application* has an objective existence. In fact, modern science shows us that force is merely a convenient term employed for the present (very usefully) to shorten what would otherwise be cumbrous expressions; but it is not to be regarded as a *thing*, any more than the bank *rate of interest* (be it 2, $2\frac{1}{2}$, or 3 per cent.) is to be looked upon as a sum of money, or than the birth-rate of a country is to be looked upon as the actual group of children born in a year. Another excellent instance is to be had from the rainfall. We say rain fell on such a day at the rate of an inch in twenty-four hours. What *can* be an inch of rain? especially when we mean a *linear*, not a *cubic* inch. But there is no confusion or absurdity here. What is implied is that, if it had gone on raining at that rate for twenty-four hours, and if the rain (like snow) remained where it fell, the ground would have been coated to the depth of an inch.

In fact, a simple mathematical operation shows us that it is precisely the same thing to say:—

The horse-power or amount of work done by an agent in each second is the product of the force into the average velocity of the agent,

and to say—

Force is the rate at which an agent does work **per unit** *of length.*

In the special illustration of Newton's words which I have just given, the resistance was a *weight*, that of a hundredweight or of a ton. When the resistance was overcome, work was done, and it was stored up for use in the raised mass—in a form which could be made use of at any future time.

Following a hint given by Young, we now employ the term ENERGY to signify the power of doing work, in *whatever* that power may consist. The raised mass, then, we say possesses, in virtue of its elevation, an amount of energy precisely equal to the work spent in raising it. This dormant, or passive, form is called *potential* energy. Excellent instances of potential energy are supplied by water at a high level, or with a "head," as it is technically called, in virtue of which it can in its descent drive machinery—by the wound-up "weights" of a clock, which in their descent keep it going for a week; by gunpowder, the chemical affinities of whose constituents are called into play by a spark, &c., &c.

Another example of it is suggested by the word "cohesion," employed in Newton's statement, and which must be taken to include what are called molecular forces in general, such as, for instance, those upon which the elasticity of a solid depends.

When we draw a bow, we do work, because the force exerted has a velocity; but the drawn bow (like the raised weight) has in potential energy the equivalent of the work so spent. That can in turn be expended upon the arrow; and *what then?*

Turn, again, to Newton's words, and we see that he speaks of one of the forms of resistance as arising from "acceleration." In fact the arrow, by its inertia, resists being set in motion; work has to be spent in propelling it, but the moving arrow has that work in store in virtue of its motion. It appears from Newton's previous statements that the measure of the rate at which work is spent in producing acceleration is *the product of the momentum into the acceleration in the direction of motion*, and the energy produced is measured by *half the product of the mass into the square of the velocity produced in it.* This active form is called *kinetic* energy, and it is the double of this to which the term *vis viva*, or *living force*, has been erroneously applied.

As instances of ordinary kinetic energy, or of mixed kinetic and potential energies, take the following:—A current of water capable of driving an undershot wheel; winds, which also are used for driving machinery; the energy of water-waves or of sound waves; the radiant energy which comes to us from the sun, whether it affect our nerves of touch or of sight (and there-

fore be called radiant heat or light) or produce chemical decomposition, as of carbonic acid and water in the leaves of plants, or of silver salts in photography (and be therefore called actinism); the energy of motion of the particles of a gas, upon which its pressure depends, &c. [When the motion is vibratory the energy is generally half potential, half kinetic.]

These explanations and definitions being premised, we can now translate Newton's words (without alteration of their meaning) into the language of modern science, as follows :—

Work done on any system of bodies (in Newton's statement the parts of any machine) *has its equivalent in work done against friction, molecular forces, or gravity, if there be no acceleration; but if there be acceleration, part of the work is expended in overcoming the resistance to acceleration, and the additional kinetic energy developed is equivalent to the work so spent.*

But we have just seen that when work is spent against molecular forces, as in drawing a bow or winding up a spring, it is stored up as potential energy. Also it is stored up in a similar form when done against gravity, as in raising a weight.

Hence it appears that, according to Newton, whenever work is spent it is stored up either as potential or as kinetic energy, except, possibly, in the case of work done against friction, about whose fate he gives us no information. Thus Newton expressly tells us that (except, possibly, when there is friction) *work is indestructible*, it is changed from one form of energy to another, and so on, but never altered in quantity. To make this beautiful statement complete, all that is requisite is to know *what becomes of work spent against friction.*

Here, of course, experiment is requisite. Newton, unfortunately, seems to have forgotten that savage men had long since been in the habit of making it whenever they wished to procure fire. The patient rubbing of two dry sticks together, or (still better) the drilling of a soft piece of wood with the slightly blunted point of a hard piece, is known to all tribes of savages as a means of setting both pieces of wood on fire. Here, then, heat is undoubtedly produced, *but it is produced by the expenditure of work.* In fact work done against friction has its equivalent in the heat produced. This Newton failed to see, and thus his grand generalisation was left, though on one point only, incomplete. The converse transformation, that of heat into work, dates back to the time of Hero at least. But the knowledge that a certain process will produce a certain result does not necessarily imply even a notion of the "why;" and Hero as little imagined that in his æolipile heat was *converted into work*, as do savages that work can be *converted into heat.*

But whenever any such conversion or transference takes place there is necessarily motion: and the mere rate of conversion or transference of energy per unit length of that motion is, in the present state of science, very conveniently called force. No confusion can arise from using such a word in such a sense. On the contrary, there is always a gain in clearness when compactness can lawfully be introduced.

Rumford and Davy, at the very end of last century, by totally different experimental processes, showed conclusively that the materiality of heat could not be maintained, and thus gave the means of completing Newton's statement which, still farther extended and generalised rather more than thirty years ago by the magnificent experimental work of Colding and Joule, now stands as one massive pillar of the fast-rising temple of science :—known as the law of the *conservation of energy.*

The conception of kinetic energy is a very simple one, at least when visible motion alone is involved. And from motion of visible masses to those motions of the particles of bodies whose energy we call heat, is by no means a very difficult mental transition. Mark, however, that heat is not the mere motions but the energy of these motions; a very different thing, for heat and kinetic energy in general are no more "*modes of motion*" than potential energy of every kind (including that of unfired gunpowder) is a "*mode of rest!*" In fact a "*mode of motion*" is, if the word motion be used in its ordinary sense, purely kinematical, not physical; and if motion be used in Newton's sense, it refers to momentum, not to energy.

The conception of potential energy, however, is not by any means so easy or direct. In fact, the apparently direct testimony of our muscular sense to the existence of force makes it at first much easier for us to conceive of force than of potential energy. *Why* two masses of matter possess potential energy when separated—in virtue of which they are conveniently said to attract one another—is still one of the most obscure problems in physics. I have not now time to enter on a discussion of the very ingenious idea of the ultramundane corpuscles, the outcome of the life-work of Le Sage, and the only even apparently hopeful attempt which has yet been made to explain the mechanism of gravitation. The most singular thing about it is that, if it be true, it will probably lead us to regard all kinds of energy as ultimately kinetic.

And a singular quasi-metaphysical argument may be raised on this point, of which I can give only the barest outline. The mutual convertibility of kinetic and potential energy shows that relations of equality (though not necessarily of identity) can exist between the two, and thus that their proper expressions involve the same fundamental units, and in the same way. Thus, as we have already seen that kinetic energy involves the unit of mass and the square of the linear unit directly, together with the square of the time unit inversely, the same must be the case with potential energy; and it seems very singular that potential energy should thus essentially involve the unit of time if it do not ultimately depend in some way on energy of motion.

[Prof. Tait then gives instances of the inaccurate use of the word Force.]

To conclude—In defence of accuracy, which is the *sine quâ non* of all science, we must be "zealous," as it were, even to "slaying." And, as all the power of the *Times* will not compel us to put a *y* instead of an *e* into the word chemist, so neither will the bad example of Germany and France, though recommended to us with all the authority which may be attributed to an ex-president of this Association, succeed in inducing us to attach two or more perfectly distinct and incompatible scientific meanings to that useful little word, "force," which Newton has once and for ever defined for us with his transcendent clearness of conception.

I have now only to ask your indulgence for the crudeness of this lecture. All I can say is that in preparing it, I have done my best, under circumstances of time, place, and surroundings, all alike unpropitious. But the chance of being able to back up, however imperfectly, my old friend, Dr. Andrews, in whose laboratory I first learned properly to use scientific apparatus, and whose sage counsel impressed upon me the paramount importance of scientific accuracy, and above all, of scientific honesty—such a chance was one which no surroundings (however unpropitious) could have induced me to forego.

NOTES

WE have received the "Daily Programme of the Twenty-fifth Meeting of the American Association," held at Buffalo, August 23–30. It forms a pamphlet of about 100 pages, but appears to have been published daily during the meetings, and is quite a model of what such a programme should be. It is clearly printed on excellent paper, and has not the overcrowded appearance that the programme of the British Association often presents. At the last meeting a standing committee was appointed to superintend the selection of papers, and to this committee a short abstract must be sent before the title of a paper can be transmitted to the sectional committees. A list of accepted papers is given each day, and appended is the time each is supposed to occupy in reading. The work of each section for each day is indicated, and all the necessary information as to officers, regulations, &c., are given. A list is also given daily of the number of members "elected" and the number "registered," with their addresses. Altogether for this meeting these amount to 352, and the number of papers entered for reading is 147. At this meeting seventeen fellows were elected, consisting of some of the best known names in American science. The next meeting of the Association will be held at Nashville, Tenn., on the last Wednesday of August, 1877, the president-elect being Prof. Simon Newcomb, of Washington.

PROF. HUXLEY was present at the meeting of the American Association for the Advancement of Science, held at Buffalo. After stating that he was quite unprepared to occupy their attention, he said :—In England we have no adequate idea of the extent of your country, its enormous resources, the distances from centre to centre of population, and we least of all understand the great basis of character which sprung from the other side of the Atlantic. There has been some talk of the influence of your climate carrying you back to the North American type.

I cannot say that I can see any signs of that unless it be in the development of that virtue of hospitality which prevails among all savages. Another feature I have observed which fills me with a certain amount of shame, when I think of what is going on in our country. I have visited your great Universities of Yale and Harvard, and have seen how your wealthy men contribute to scientific institutions in a way to which we are totally unaccustomed in England. The general notion of an Englishman who becomes rich is to buy an estate and found a family. The general notion of an American who becomes rich is to do something for the benefit of the people, and to found an institution whose benefits shall flow to all. I need hardly say which I regard as the noblest of these two. It is commonly said there are no antiquities in America, and you have to come to the Old World to see the past. This may be, so far as regards the trumpery of 3,000 or 4,000 years of human history. But, in the larger sense America is the country to study antiquity. I confess that the reality somewhat exceeded my expectations. It was my great good fortune to study in Newhaven the excellent collection made by my good friend, Prof. Marsh. There does not exist in Europe anything approaching it as regards extent and the geological time it covers, and the light it throws on the wonderful problem of evolution which has been so ably discussed before you by Prof. Morse, and which has occupied so much attention since Darwin's great work on species. Before the gathering of such materials as those to which I have referred, evolution was a matter of speculation and argument, though we who had adhered to the doctrine had good grounds for our belief. Now things are changed, and it has become a matter of fact and history. The history of evolution, as a matter of fact, is now distinctly traceable. We know it has happened, and what remains is the subordinate question of how it happened. I wish you all good speed, and that this Association, like its sister in Great Britain, will sow the seeds of scientific inquiry in all the towns it visits, and thus help on the great good work."

ON Tuesday the proceedings of the Iron and Steel Institute were formally opened at Leeds, Mr. W. Menelaus, President, in the chair. The choice of President for the ensuing Session has fallen upon Dr. C. W. Siemens, F.R.S. The geological features of the neighbourhood of Leeds was the subject of an interesting and valuable paper by Prof. Green, F.G.S., of the Yorkshire College of Science, read by the Secretary. The paper was descriptive of the various geological formations of the district, the coal measures and the iron deposits being specially referred to. Referring to the coal mines of the district, he observed that there was an area of forty square miles upon which no coal had as yet been raised, although it was well known that coal-seams existed beneath the surface. A very small proportion only of the Yorkshire coal-fields had as yet been worked, a large area remaining untouched which contained the vast store of coal for future use. A paper was read by Mr. Dove, junior, on the North Lincolnshire Iron District. The author described the new iron district of North Lincolnshire, the centre of which is Frodingham. It appears the district has only been known for the past fifteen years, during which time its rise and development have been steady and rapid. The question of the open *versus* close-topped blast-furnaces was then discussed. Mr. J. Lowthian Bell, M.P., observed that as far as economy of fuel in the smelting process was concerned, there was not much to choose between the two systems in ordinary practice; where the real economy lay in close-topped furnaces was in the utilisation of the gases from the furnace for heating the steam boilers and the stoves. Mr. John Jones, the secretary of the Institute, then read a paper on technical education in connection with the iron trade. He observed that the great hindrance which had hitherto been experienced in dealing with technical education had been the unsatisfactory condition of primary education in England. Most of the time of the meeting during the week will be occupied in visiting the various industrial establishments in Leeds and neighbourhood.

AMONG the papers read at the Oriental Congress, in addition to those already mentioned, are the following:—Mr. Smirnow read an account of a Turkish MS. in the University Library of St. Petersburg, "On the Mythology of the Asiatic Peoples." The age of this MS. he thinks to be the 17th century. Prof. de Rosny then discussed with much learning and at considerable length the comparative philology of the languages vaguely styled Turanian, the meaning and application of which term he critically investigated. He thought there was as safe a basis for the scientific classification of these tongues—comprising the Chinese, Japanese, Tatar, Finnish, Basque, &c.—as there was acknowledged to be for that of the Aryan and Semitic languages. M. Slovstof read an interesting paper on "The History of Public Instruction in Western Siberia," and M. Neumann one on the Tchouktchis, a generic name for three different peoples who inhabit the whole of North-East Siberia—viz., (1) the Rennes, (2) the Aigwanes, (3) the Nammolo. M. Sobruk, an Ostiak gentleman, read a memoir of great interest on the idols of his people and the Voguls, which, however, were no longer worshipped in public, or at least very rarely. Those which exist are confined to the huts of the believers. M. Solovief gave an ethnographical survey of the Samoied tribes of Siberia. Mr. Bonnell introduced the subject of the Scytho-Sarmatians and other inhabitants of the coasts of the Euxine, whose fortunes and history he elaborately traced in the pages of successive chroniclers, beginning with Herodotus. A communication by M. Schmidt, of Gevelsberg, after tracing the origin of Egyptian civilisation to Mesopotamia, from which it migrated to the Nile across the Persian Gulf, by way of Arabia and Ethiopia, was chiefly interesting for the attention he drew to the striking analogies in the languages of the American tribes with those of the Armeno-Caucasians, which were altogether too intimate, too frequent, and too decided to allow of the entertainment of any hypothesis of accidental similarity. From this suggestive line of thought M. Schmidt passed on to the consideration of the ethnological antiquities of the Medes, the bulk of whom he believed to be an Iranian people. M. Oppert developed his ideas upon the cuneiform texts written in the language of ancient Armenia and called Armeniac, but having no affinities with the language known as Armenian. M. Sachau strongly urged the importance of studying the scientific literature of the Arabs, and praised most warmly the services rendered to such studies by the publications of the St. Petersburg Academy, instancing the translation of Abderahman al Sufi's "Description of the Fixed Stars." Many other subjects of importance, mainly relating to the traditions, mythology, history, and literature of the varied peoples of the vast Russian territory, were discussed. The Congress will hold its next meeting at Florence.

THE paper by Mr. J. A. Broun "On Simultaneous Variations of the Barometer" (*Proc.* Roy. Soc., No. 171, 1876), is remarkable as raising the inquiry whether there may not be other causes of varying atmospheric pressure than change of the mass of air, in other words, whether the attraction of gravitation be the only force concerned in the barometric oscillations. It is shown from observations made at places in Europe, Asia, Australasia, Africa, and America during the week March 31 to April 5, 1845, that all the curves exhibit a maximum near the beginning and another near the end of the week, with a minimum near the middle, and it is inferred that we have here an indication of the general action of the same cause of barometric variation over the earth. Since it would be impossible to over-estimate the importance of the point here raised if it should turn out to be correct, we shall look forward with much interest to the further investigation of the subject promised us by Mr. Broun. In this connection the

International Charts of General Myer, of the United States, will be of the very greatest value.

IN a recent number of *Poggendorff's Annalen*, Dr. G. Berthold makes an interesting contribution to the history of the radiometer. It appears that in a paper entitled "Eclaircissement sur le traité physique et historique de l'aurore boréale," published in the *Memoirs* of the Paris Academy for 1747, M. Mairan gives a description of a light mill. This was a horizontal wheel of iron about 3 inches in diameter, having six radii; at the end of each radius was a small oblique vane. The axis of the wheel was held by its upper point to the end of a magnetic bar. The weight was only thirty grains. Light was concentrated on it with a lens. "Nothing could be more mobile," says M. Mairan, "than this wheel; but at the same time nothing is less certain than the induction one might wish to draw from it in favour of an impulsion by the rays. The machine turns now in one direction, now in the other, according as you bring one of its vanes more or less near to the bars, within, or beyond the latter. It is necessary to conclude that the luminous rays attract and repel at different points of the cone which is formed by the lens, but the explosion of a mass of air suddenly and unequally heated round the vane where the focus is applied, appears to me to give a sufficient reason for these effects. The perpetual obstacle of the air naturally suggested to me to make one of these experiments *in vacuo*, but I avow that after having reflected a little on what might be the result, I have not thought it worth while taking the trouble." The reasons which thus unfortunately prevented M. Mairan from repeating his experiments *in vacuo* were, (1) the difficulty of producing a sufficient vacuum; (2) the idea, that besides the atmospheric air there was another fluid, which would penetrate the glass and make the experiment doubtful; (3) through action of the burning-glass vapours would rise from the body *in vacuo*, which would, by their impulsion, set it in motion. Dr. Berthold further notices an observation by Michell in Priestley's "History of Optics;" a piece of piano string, 10 in. long, having a square copper plate at one end, and a grain of shot at the other, was pivoted in a case having its cover and one side of glass. Solar rays directed from a concave mirror on the copper plate produced repulsion. Priestly considered that this motion must not be attributed to impact of the light rays.

THE French *Journal Official* publishes a letter from Shanghai stating that a Chinese Polytechnic Institution, supported by private contributions, has been opened there.

PETERMANN'S *Mittheilungen* for September contains some papers of great interest. Dr. Hermann Wagner, of Königsberg, contributes a careful and detailed *résumé* of the most recent trustworthy contributions to a knowledge of the Bolivian littoral, its physical features, products, and people, accompanied by a map. Oscar Loew gives the results of Lieut. Wheeler's expedition in California, Nevada, and Arizona for the year 1875, the results being embodied in a map by Dr. Petermann. Lieut. Weyprecht's "Pictures from the High North" are continued, the present instalment giving an interesting account of the behaviour of the sailors of the expedition in the ice. The Brazilian engineer, Maximilian Emerich, describes the various projects that have been proposed for a South American Pacific railway, and Dr. Mupperg, of Venice, contributes a picturesque paper on the German element, which is very strong, in Italy, especially in South Tyrol.

A HURRICANE burst over St. Thomas and St. Croix on the night of the 12th inst. The damage done was not extensive. Rain fell in torrents the whole time.

SNOW has been observed not only in Scotland on the Grampians, but on the Observatory of Puy-de-Dôme, on the 12th instant, and on the Alps round St. Jeanne, Maurienne, and other places, about the same time.

HER Majesty has directed letters patent to be passed under the Great Seal declaring that the degrees of Bachelor and Master in Arts, and Bachelor and Doctor in Law, Medicine, and Music, hereafter to be granted or conferred by the University of New Zealand, shall be recognised as academic distinctions and rewards of merit, and be entitled to rank, precedence, and consideration in the United Kingdom, and in the colonies and possessions of the crown throughout the world, as fully as if the said degrees had been granted by any university of the said United Kingdom.

THE Dutch Government has ordered from the French International Metric Commission, a copy of the standard metre, to be executed at its own expense. The same thing has been done already for the English Government.

ON Oct. 1 the first number of *The Sunday Review* will be published by Trübner and Co. It will be a shilling quarterly magazine, the organ of the Sunday Society, whose object, our readers know, is to obtain the opening of museums, art galleries, libraries, aquariums, and gardens on Sundays.

PART I., Vol. 1, of the *Proceedings* of the West London Scientific Association has been published. It contains the inaugural address of the President, the Rev. G. Henslow, for 1875-6, and a report of the meetings to the end of last year.

THE system of forest conservancy which is proving so satisfactory in India, is becoming imitated more or less in various parts of the world. In the Vilayet of Trebizond the virgin forests cover an area of 1,000 square miles, one half of which belongs to the crown, and the other half, which consists mainly of groves, situated in the vicinity of villages, is considered by the inhabitants as belonging to the commons, and in a few cases to private individuals. Most of the crown forests are in the districts of Livanah, Adjarah, Batoom, Tsorook Soo, and Off on the east; of Trebizond, Ordoo, Guerela, and Aktshe Abad on the west; and Madjka, Kurtine, Kelkit, and Shagran on the south. In these forests the pitch-pine, fir, ordinary pine, and beech predominate. Chestnut, alder, elm, oak, ash, maple, and lime are also everywhere and in great numbers. Boxwood grows especially at Alina and Rijah, and the juniper at Kerasond, Tinebali, and Livanah. The mean distance of the forests from the sea-shore is about fifteen miles. Although in general the means of transport are wanting, there are many forests that can be worked with comparatively small outlays for the construction of short roads, in consequence of the proximity to the existing high roads, such as the forests of Kerasond and Madjka, or to rivers on which the timber can be floated, such as the forests of Livanah and Sireboli. With the exception, however, of boxwood, exported from Kiyeh and Atinah, and a little timber from Batoom, no advantage is derived at present from the extensive forests belonging to the crown. The timber and fire-wood used for local consumption is usually cut in the groves situated at no great distance from the sea-shore, or near the villages, and which are claimed by the inhabitants as belonging to them. In consequence of these woods having been constantly and indiscriminately felled, and often burnt down for the purpose of obtaining arable land, they are in a very poor condition. Of late, however, to prevent this destruction, forest guards have been appointed under the orders of special officers.

THE Paris observatory has been opened again to public inspection, on Thursday evenings. Applications must be made by letter to the Secretary.

SIGNOR D'ALBERTIS left Somerset, in York Peninsula, on May 18th last, on his exploring expedition to New Guinea.

He anchored at Harvey's Reef on the same night and left for Long Island on the following morning. Writing on May 21st, he hoped to be in the Fly River in two days more. He has obtained a parrot which he thinks is new—an *Eclectus*. In the July number of the *Melbourne Review*, an article by Dr. G. Bennett contains a life of Signor D'Albertis, together with an account of his journey to the Arfak Mountains.

IT will interest zoologists to know that living specimens of the fish *Ceratodus* have been received at Sydney from Maryborough, in Queensland, and that there is some prospect of their reaching the Zoological Society's Gardens in Regent's Park.

DR. MIKLUCHO MACLAY, the Russian naturalist, is returning to his old field of scientific research in Astrolabe Bay, on the north-east coast of New Guinea, and he desires that passing ships should give him a call.

A CORRESPONDENT of the *Times* describes two thoroughly prehistoric spectacles which he witnessed in Fiji. One was a young girl dressed in two yards of calico print and a girdle of leaves, breaking "ivi" nuts—a kind of large coarse chestnut with a hard shell—with a genuine stone adze, fixed to its wooden handle by coils of plaited string. The other was a little shrivelled old woman, who was making an earthenware vessel, nearly as large as herself, with no other implements than a round flattened pebble about four inches in diameter, and a piece of wood about as large as the back of an ordinary hair brush, slightly concave on the surface. Dipping both stone and wood frequently in water she moulded the inside of the huge pot with the former, and patted the outside into shape simultaneously with the latter. The vessel was egg-shaped, the opening being at the top or large end of the egg with an everted lip. It was nearly three feet in height and two in diameter, and was formed of clay found near the village. When it is complete a fire is built round it on the ground, and it is carefully baked before being removed. In the houses these pots are placed on their side with the mouth inclined slightly upwards, and are seldom exposed to the risk of breakage by removal from their side. They are, of course, very fragile, but in the hands of the natives they are said to last for years.

THE International Geographical Congress at Brussels, which concluded its labours last Thursday, has drawn up a programme relative to African exploration, in which it is recognised as necessary that stations should be established for the purpose of furnishing travellers with the means of existence. An international committee and branch committees in each country are to be appointed. The International Executive Committee will be composed of Sir Bartle Frere, Dr. Nachtigal, and M. Quatrefages, and will be presided over for the first year by the King of the Belgians, with the idea of allowing the presidency to pass successively to distinguished personages of other countries.

THE *Boston Medical and Surgical Journal* contains a short account of the late distinguished naturalist, Christian Gottfried Ehrenberg, whose death we announced last week. Born in 1795, at Delitzsch, he commenced the study of theology at Leipsic when twenty years of age. In 1817 he matriculated at Berlin, and devoted most of his time to physiological chemistry. Between 1818 and 1820 he spent much time in the study of the fungi. During the five years following he travelled in Egypt and Arabia. In 1829 he accompanied von Humboldt to the Ural Mountains. Between that time and 1834, under the title, "Symbolæ Physicæ," he published contributions to the anatomy and physiology of the lower invertebrata. In 1835 he published a paper on phosphorescence, which he explained as dependent on the presence of infusoria; and shortly afterwards his works on "Infusoria as Perfect Organisms," and "A Glance at the Deeper Life of Organic Nature," appeared. In 1837 he was elected a Fellow of the Royal Society of England, and, in 1842, one of the thirty Knights of the Order of the Friedens Klasse. During the latter part of his life Ehrenberg suffered from cataract, a successful operation for the removal of which he survived but a fewn weeks.

AT the meeting of the Academy of Medicine in Paris, on August 8, M. Broca read a memoir on cerebral topography, in which, among other points, he showed that Gratiolet was misled in supposing that the fissure of Rolando coincides with the coronal suture of the skull. M. Broca seems to be unacquainted with Prof. Turner's investigations in this direction, which demonstrate that the fissure of Rolando lies as much as 1½ or 2 inches behind the coronal suture.

IN a paper of considerable interest in the *Journal of the Asiatic Society of Bengal*, vol. xlv. part 2, 1876, on protracted irregularities of atmospheric pressure, and their relation to variations of the local rainfall, Mr. H. F. Blanford is led to conclude that the distribution of pressure in India is subject to protracted local variations, which are nevertheless not permanent, and that these irregularities of pressure probably explain the irregularities of the rainfall. The former of these may almost be regarded as an established fact in Indian meteorology, while the latter can as yet be regarded as only probable. For the elucidation of this highly practical and scientific question, longer continued observations, and observations embracing a wider extent of the monsoon region, are required than are yet available.

WE have on our table the following books:—"Field Geology," W. H. Penning (Baillière, Tindall, and Cox). "Central Africa," Col. C. Chaillé Long (Sampson Low and Co.). "Electro-Telegraphy," F. S. Beechey (Spon). "The Theory of Sound and its Relation to Music," Prof. Pietro Blaserna (International Scientific Series: H. S. King and Co.). "Catalogue of the Western Scottish Fossils" (Blackie and Son). "Notes on the Fauna and Flora of the West of Scotland" (Blackie and Son). "The Principal Manufactures of the West of Scotland" (Blackie and Sons).

ALL the tanks at the Royal Westminster Aquarium are now complete and stocked. It is estimated that the entire exhibition of marine and fresh-water animals embraces no less than fifteen thousand individuals, representing one hundred and thirty-seven distinct varieties. Out of these the class of fishes includes eighty-five species and thirteen thousand specimens. Among the latest arrivals are several examples of the Spanish Bream (*Pagellus erythrinus*), now for the first time exhibited in this country; six specimens of the John Dory (*Zeus faber*), and a shoal of Boar-fish (*Capros aper*). The reptilian section has been enriched by a specimen of the true tortoise-shell producing turtle (*Caretta imbricata*). It is proposed shortly to commence a series of popular lectures upon the inhabitants of the tanks.

THE additions to the Zoological Society's Gardens during the past week include a Pig-tailed Monkey (*Macacus nemestrinus*) from Java, presented by Mr. Meyrick; a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. Edward Soy; a Black-eared Marmoset (*Hapale penicillata*) from south-east Brazil, presented by Miss Woellwarth; a Coati (*Nasua nasica*) from South America, presented by Dr. C. R. Bree; a Common Raccoon (*Procyon lotor*) from Central America, presented by Mr. H. B. Whitmarsh; a King Parrakeet (*Aprosmictus scapulatus*) from Australia, presented by Mr. H. T. Sissons; a Rüppell's Spur-winged Goose (*Plectropterus rüppelli*) from East Africa, presented by Mr. M. J. M. Cornely; a Burchell's Zebra (*Equus burchelli*) from South Africa, two Hairy Armadillos (*Dasypus villosus*) from La Plata, deposited; two Russell's Vipers (*Vipera russelli*) born in the Gardens.

SCIENTIFIC SERIALS

Poggendorff's Annalen der Physik und Chemie, No. 6, 1876.—From experiments it is here inferred by Dr. Buff, of Giessen, that the heat conductivity of hydrogen and other gases is too small to be demonstrable by the method proposed by Magnus. Hence the supposition of a conductivity similar to that of metals (if aught more is meant, than that hydrogen can, like solid and liquid bodies, transfer heat from molecule to molecule), is unwarranted. On the other hand, hydrogen has a penetrability for heat rays which comes very near that of vacuum. Dry air absorbs 50 to 60 per cent. of heat rays from a source heated to the boiling point of water. The absorptive power of moist air exceeds that of dry air by several percentages, but not nearly so much as has been supposed by some physicists. Rock salt is not perfectly diathermanous to so-called obscure heat rays. Its "heat colour" is rather like that of dry air.—Dr. H. C. Vogel describes some interesting experiments on change in pitch of tone of a moving body; they consisted in observation of the whistle of a locomotive, and the results closely agree with Doppler's theory and calculations.—M. Wiedemann's paper on the laws of passage of electricity through gases is here concluded. The experiments relate to difference of effect according as positive or negative electrode (in the discharge apparatus) is connected to earth, effect of varying length and width of tube between the electrodes, also of varying pressure and gas, the rise of temperature produced by the discharge, effect of heating electrodes, &c. The view M. Wiedemann adopts is, that in discharge, the gas molecules on the electrodes carry off electricity with them, and impart it to others against which they are driven, and these in their turn are impelled against a third set, and so on; the case being similar to that of a row of freely suspended elastic balls, one of the end ones of which is driven against its neighbour. The author further studies the unequal expansion of the positive and negative discharge, the place where the *vis viva* of the moved gas masses is finally transformed into heat, the dark space at the negative electrode and the stratification of the light, and points out the relation in which his results stand to those obtained by Hittorf.—The constants of dielectricity of oil of turpentine, benzol, and two varieties of petroleum, are determined by M. Silow, by the condenser method, and their square roots are shown to correspond closely to the refractive indices of the liquids, with $\lambda = \infty$ (according to Maxwell's law).—Some anomalous phenomena of the gold-leaf electroscope are pointed out by M. Beetz (they indicate a streaming out of electricity from the leaves over the glass).—We note, lastly, a paper of contributions from the Mineralogical Institute of Strasburg University, referring to glaucophane, datolith, safrol, crystalline form and optical properties of isomerous dinitro benzol, &c.

Journal of the Chemical Society, June.—This number contains an extensive and exceedingly interesting paper on some points in the analysis of potable waters, by Prof. Frankland, D.C.L., F.R.S. Some eight years since, Dr. Frankland, in conjunction with Dr. Armstrong, laid before the Fellows of the Chemical Society an account of the observations and experiments made by them during two years on the methods then employed in the analysis of potable waters. During the time which has elapsed since that occasion Dr. Frankland has adopted the combustion and collateral processes then recommended, and nearly nine years' further experience in water analysis induce him to claim for this process the following recommendations:—1. It is the only process which affords trustworthy information respecting the organic matters present in potable water. 2. It alone professes to determine organic carbon in such waters. 3. Its method of determining organic carbon and nitrogen gives fairly accurate results, even in the hands of a comparatively inexperienced analyst. 4. It alone discloses the proportion of nitrogen to carbon in the organic matter of waters. 5. The process can now be conducted in any laboratory with little difficulty, owing to the modifications in the method of evaporation which have been made.—Mr. W. H. Perkin, F.R.S., contributes a paper upon the formation of anthrapurpurin.—Dr. Thorpe, F.R.S., communicates some notes from the laboratory of the Yorkshire College of Science, Leeds, comprising a short paper by Herbert Eccles on the action of the copper-zinc couple on potassium chlorate and perchlorate, one by John Muir on thallium chlorate, and a third by Dr. Thorpe himself, on the isometric relations of thallium. As usual the remainder of this volume contains numerous abstracts of chemical papers published in British and foreign journals.

THE *Jahresbericht*, 1874-5, of the Swiss *Naturforschende Gesellschaft*, contains a lengthy account of this Society's last annual meeting, held at Andermatt in September 1875. The opening speech was delivered by Prof. Kaufmann, the president, and mainly of geological interest. Amongst a number of smaller papers that were read we note the following more important ones: On the observations of temperature made in the St. Gotthard tunnel; the temperatures of air, water, and of the soil were registered at a great number of different places in the tunnel, as far as it is constructed, both on the north and south sides, by Dr. Stapff.—On the so-called "seiches," oscillation waves observed in Swiss lakes, principally Lake Leman, by Dr. Forel.—On the recent appearance and the damage done by locusts in the east Swiss Rhine districts, and on the banks of the Bieler Lake, by Prof. C. G. Brügger and Alb. Müller.—The other papers are of minor interest.

SOCIETIES AND ACADEMIES

NEW SOUTH WALES

Royal Society, May 17.—Rev. W. B. Clarke, M.A., F.G.S., in the chair.—The officers for the ensuing year were balloted for:—President (*ex-officio*), the Governor, Sir Hercules Robinson, K.C.M.G., &c.; Vice-presidents, the Rev. W. B. Clarke, M.A., F.G.S., Mr. H. C. Russell, F.R.A.S., Government Astronomer; Hon. Secretaries, Prof. Liversidge, Dr. Leibius. The treasurer presented his annual statement, which showed that although the Society had expended a considerable sum during the past year upon furniture and fittings for the new rooms, there was still a very satisfactory cash balance. The Rev. W. B. Clarke then delivered his annual address. The Society was informed that sections were about to be established by the council in order that members who devoted themselves to particular branches of scientific study might have afforded to them more frequent opportunities for meeting and working together than was possible at the more formal general meetings of the Society.

June 7.—The Rev. W. B. Clarke, F.R.S., in the chair.—The chairman stated that the deputation appointed for the purpose at a former meeting, had waited on the Minister for Justice and Public Instruction and had submitted a request to be communicated to the Government for the sum of 3,500*l*. for the erection of a suitable building and 300*l*. annuity for the ordinary purposes of the Society. They were courteously received, and the Minister cordially promised to lay the matter before his colleagues.—Prof. Liversidge, hon. secretary, announced that a large number of members had entered their names for the sections, and gave notice that arrangements had been made for the preliminary meetings of the following sections, viz.:—Section A. Astronomical and Physical Science. B. Chemistry and Mineralogy. C. Geology and Palæontology. D. Biology. E. Microscopical Science. F. Geography and Ethnology. G. Literature and Fine Arts. H. Medical Science. I. Sanitary and Social Science and Statistics. It was mentioned that a large number of gentlemen interested in scientific matters were desirous to be elected into the Society as soon as the above sections were established.—Mr. H. C. Russell, F.R.A.S., Government Astronomer, then read a paper entitled, "Notes upon some Remarkable Errors in Thermometers," which had been exhibited by standard instruments at the observatory. He also exhibited an improved form of heliostat suitable for signalling purposes.

GÖTTINGEN

Royal Academy of Sciences, March 4.—The following, among other papers, were read:—Some important improvements in simple and compound influence-machines, by M. Holtz.—On the constitution of steel and its connection with magnetisability, by M. Fromme. He credits M. Ruths with the true settlement of this question. With small magnetising forces an annealed bar always takes more magnetism than a similar hardened bar. But as the magnetism in the hardened bars increases in greater ratio than in annealed bars, a value of magnetising force is reached, at which the magnetism of the hardened bar reaches that of the annealed, thereafter exceeding it. This indifferent force is smaller the thicker the bar in comparison to its length. With a certain ratio of length and thickness it becomes infinitely great. The contradictions of previous observers are explained when dimensions are taken into account. M. Fromme, using more adequate means, confirmed M. Ruths' results. M. Gaugain has recently got results that fully agree with those of Ruths; but M. Fromme explains them somewhat differently from the French physicist.

April 12.—Contribution to anatomy of the medullated peripheral nerve fibres, by M. Kuhnt.

May 6.—On the conductivity of electrolytes dissolved in water, in connection with the wandering of their constituents, by M. Kohlrausch. The conductivities of electro-chemically equivalent solutions of two electrolytes which have one constituent in common, are inversely as the transference-numbers of the same equivalent; or the product of the conductivity of the solution and the transference number of the common constituent on both sides is the same. The hindrances to movement in dense solutions, it is found, generally affect the kation more than the anion.—On the movement of electricity in material conductors, especially in a conducting ball, by M. Riecke.—Sulphide of carbon as a preserving and disinfecting substance, by M. Zoeller.—On the pressure forces arising from simultaneous motions associated with contractions and dilatations of several spherical bodies in an incompressible liquid, by M. Bjerknes.

June 17.—Theory of unipolar induction and Plücker's experiments, by M. Riecke. He considers first, the induction of a moved magnetic pole on a linear conductor at rest; then the induction of a magnetic pole at rest on a rotating conductor; then applies the principles arrived at to Plücker's experiments; a fourth chapter is on Wilhelm Weber's unipolar induction.—Contributions to anatomy of the Crinoideæ (second article), by M. Ludwig.—Physiology and histology of the central nerve-system of helix pomatia, by M. v. Ihering.—Sulphide of carbon as a preserving substance (second paper), by M. Zoeller. Five drops of the liquid to a litre of air space suffices to preserve the most decomposable fruits and vegetables. These tasted quite fresh after short exposure to the air, and meat quite lost the smell of sulphide of carbon after boiling or roasting, but it had a slight flavour like that of game, which, to most people, is not unpleasant. It appears that sulphide of carbon acts in the way of coagulating albuminous substances and lessens the water-contents of the preserved substances.

VIENNA

Imperial Academy of Sciences, July 6.—On the causes of keratitis after section of the trigeminus, by Dr. Feuer.—Experiments on the heat conductivity of nitrogen, binoxide of nitrogen, ammonia, and coal-gas, by M. Plank. These are, respectively (the conductivity of air being made = 1), 0·993, 0·951, 0·917, 2·670.—Studies on the more recent tertiary formations of Greenland, by M. Fuchs. Several new fossil species are described.

July 13.—Action of current electricity on the motion of protoplasm, on living and dead cell contents, and on material particles generally. Second part: Influence of the galvanic current on dead cell contents; by M. Velten. Very strong induction currents sent through a cell, or a number of cells, set the contents in rotation, which is very like vital rotation, and follows the same laws. The botanical phenomena of circulation, sliding motion, &c., can be well imitated by this means. M. Velten infers that the cause of protoplasm-motions is to be sought in electric currents produced in the living cell contents.—On the advancement of science by professors and private *savans*, the doctrine of geognostic land-types, and the method of geological surmises *à priori*, by M. Boué.—On some elementary infinite series, by M. Igel.

PARIS

Academy of Sciences, Sept. 4.—Vice-Admiral Paris in the chair.—The following papers were read:—New theorems relative to couples of segments making a constant length, by M. Chasles.—Researches on the disappearance of ammonia contained in waters (first part), by M. Houzeau. Water from wells quickly loses its ammoniacal principle in a vessel hermetically sealed. Light favours this disappearance, but is not indispensable to the phenomenon. This suggests the practical process of exposure to the sun. M. Houzeau also found that artificial ammonia added to water (in the form of carbonate of ammonia) quickly disappeared.—Representation of elliptical functions of the first species by means of left biquadratics. Extract from memoir by M. Léauté.—Rectification of a previous communication on determination by the principle of geometrical correspondence, of the order of a geometrical place defined by algebraic conditions, by M. Saltel.—Results obtained by means of new apparatus for extraction of the juices of sugar-cane, by MM. Mignon and Rouart. The plan they have adopted (in Guadaloupe) is partly like that in treatment of beet. They use a rasp or defibrating machine; this process reaches the hardest parts forming the envelope of the cane, and disorganises the cells which are richest in sugar and which most easily escape in the ordinary treatment. In the hydraulic press used, there are two pistons; the smaller gives twelve atmospheres, and acts during the whole of the compression; the action of the larger piston is added, the two together giving a pressure of 80 atmospheres. The results obtained surpass considerably those from ordinary methods. Thus cane simply defibrated and subjected to only one pressure, gave 77 per cent. of its weight of very rich saccharine juice.—On a submarine elevation observed in the Gulf of Arta, by M. de Cigalla. In 1847 and 1865, after some shocks of earthquake, a very dense sulphurous vapour rose from the bottom and destroyed many fishes (such emanations still occur, but less in quantity). The hydrographic maps for 1847 gave 8 fathoms as the depth there. Now recent soundings show that the bottom has risen, forming a cone 300 fathoms in circumference, and with its summit of 2 fathoms 4 feet under the surface. The temperature of the water is not sensibly altered. Objects kept in the water a few days are covered with a light coat of sulphur. The raised ground consists of very small shells, while the neighbouring bottom is of oozy nature.—Observation of American vines attacked by phylloxera, in the environs of Stuttgardt, by M. Schnetzler. Three centres of invasion were discovered in July. The vines infested are all of American origin, and were imported twelve or thirteen years ago, either directly from America, or from France. The insect attacks the roots and rootlets.—Observations of the planet 166, by Mr. Peters.—Discovery of planet 167,—dispatch transmitted on Aug. 29, 1876, by Mr. Joseph Henry, of Washington. The planet was discovered by Mr. Peters of Clinton.—On the characteristics of conical systems, by M. Halphen.—New theory of the numbers of Bernoulli and Euler, by M. Lucas.—On the invention of the pneumatic fire-syringe, by M. Govi. From the *Giornale dei Letterati*, published in Rome about the middle of the eighteenth century, it is shown that the pneumatic fire-syringe, which has been thought to date from 1862 or 1863, was invented and described in 1745 by the Abbé Augustin Ruffo, of Verona, more than half a century before a workman of St. Etienne gave the idea of it to Prof. Mollet, of Lyon, or M. Fletcher experimented with it before Mr. Nicholson.—On the dissociation of bicarbonate of soda at the temperature of 100°, reply to M. Gautier, by M. Urbain. M. Gautier, heating 4 grammes of dry bicarbonate of soda between 100° and 115°, found it completely decomposed in eighteen hours; he infers that in dried blood-plasma, thus heated, the bicarbonate of soda must also be decomposed. M. Urbain denies the inference, because in the latter case the salt is empasted in a substance which forms a varnish round each of its fragments, and this corresponds to the case of heating the salt in a closed vessel, when dissociation does not occur.—Note on the phenomena of digestion in the American Cockroach (*Periplaneta americana*, L.), by M. Plateau. His examination of this insect confirms his former observations, from which he concluded that the digestive juices of insects are alkaline or neutral, never acid.—Researches on the silicified plants of Autun and Saint Etienne; Calamodendreæ and their probable botanical affinities, by M. Renault. Several resemblances seem to favour the supposition that Calamodendreæ have been the ancestors of the present Gnetaceæ.

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